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ART. XXXV.—*On Radiant Matter: A Lecture delivered to the British Association for the Advancement of Science, at Sheffield, Friday, August* 22, 1879; by WILLIAM CROOKES, F.R.S.

To throw light on the title of this lecture I must go back more than sixty years—to 1816. Faraday, then a mere student and ardent experimentalist, was 24 years old, and at this early period of his career he delivered a series of lectures on the General Properties of Matter, and one of them bore the remarkable title, *On Radiant Matter*. The great philosopher's notes of this lecture are to be found in Dr. Bence Jones's "Life and Letters of Faraday," and I will here quote a passage in which he first employs the expression *Radiant Matter*:—

"If we conceive a change as far beyond vaporization as that is above fluidity, and then take into account also the proportional increased extent of alteration as the changes rise, we shall perhaps, if we can form any conception at all, not fall far short of Radiant Matter; and as in the last conversion many qualities were lost, so here also many more would disappear."

Faraday was evidently engrossed with this far-reaching speculation, for three years later—in 1819—we find him bringing fresh evidence and argument to strengthen his startling hypothesis. His notes are now more extended, and they show that in the intervening three years he had thought much and deeply on this higher form of matter. He first points out that matter may be classed into four states—solid, liquid, gaseous and radiant—these modifications depending upon differences

in their several essential properties. He admits that the existence of Radiant Matter is as yet unproved, and then proceeds, in a series of ingenious analogical arguments, to show the probability of its existence.*

If, in the beginning of this century, we had asked, What is a Gas? the answer then would have been that it is matter, expanded and rarefied to such an extent as to be impalpable, save when set in violent motion; invisible, incapable of assuming or of being reduced into any definite form like solids, or of forming drops like liquids; always ready to expand where no resistance is offered, and to contract on being subjected to pressure. Sixty years ago such were the chief attributes assigned to gases. Modern research, however, has greatly enlarged and modified our views on the constitution of these elastic fluids. Gases are now considered to be composed of an almost infinite number of small particles or molecules, which are constantly moving in every direction with velocities of all conceivable magnitudes. As these molecules are exceedingly numerous, it follows that no molecule can move far in any direction without coming in contact with some other molecule. But if we exhaust the air or gas contained in a closed vessel, the number of molecules becomes diminished, and the distance through which any one of them can move without coming in contact with another is increased, the length of the mean free path being inversely proportional to the number of molecules present. The further this process is carried the longer becomes the average distance a molecule can travel before entering into

*"I may now notice a curious progression in physical properties accompanying changes of form, and which is perhaps sufficient to induce, in the inventive and sanguine philosopher, a considerable degree of belief in the association of the radiant form with the others in the set of changes I have mentioned.

"As we ascend from the solid to the fluid and gaseous states, physical properties diminish in number and variety, each state losing some of those which belonged to the preceding state. When solids are converted into fluids, all the varieties of hardness and softness are necessarily lost. Crystalline and other shapes are destroyed. Opacity and color frequently give way to a colorless transparency, and a general mobility of particles is conferred.

"Passing onward to the gaseous state, still more of the evident characters of bodies are annihilated. The immense differences in their weight almost disappear; the remains of difference in color that were left, are lost. Transparency becomes universal, and they are all elastic. They now form but one set of substances, and the varieties of density, hardness, opacity, color, elasticity and form, which render the number of solids and fluids almost infinite, are now supplied by a few slight variations in weight, and some unimportant shades of color.

"To those, therefore, who admit the radiant form of matter, no difficulty exists in the simplicity of the properties it possesses, but rather an argument in their favor. These persons show you a gradual resignation of properties in the matter we can appreciate as the matter ascends in the scale of forms, and they would be surprised if that effect were to cease at the gaseous state. They point out the greater exertions which Nature makes at each step of the change, and think that, consistently, it ought to be greatest in the passage from the gaseous to the radiant form."—*Life and Letters of Faraday*, vol. i, p. 308.

collision; or, in other words, the longer its mean free path, the more the physical properties of the gas or air are modified. Thus, at a certain point, the phenomena of the radiometer become possible, and on pushing the rarefaction still further, i. e., decreasing the number of molecules in a given space and lengthening their mean free path, the experimental results are obtainable to which I am now about to call your attention. So distinct are these phenomena from anything which occurs in air or gas at the ordinary tension, that we are led to assume that we are here brought face to face with Matter in a Fourth state or condition, a condition as far removed from the state of gas as a gas is from a liquid.

Mean Free Path. Radiant Matter.

I have long believed that a well-known appearance observed in vacuum tubes is closely related to the phenomena of the mean free path of the molecules. When the negative pole is examined while the discharge from an induction-coil is passing through an exhausted tube, a dark space is seen to surround it. This dark space is found to increase and diminish as the vacuum is varied, in the same way that the mean free path of the molecules lengthens and contracts. As the one is perceived by the mind's eye to get greater, so the other is seen by the bodily eye to increase in size; and if the vacuum is insufficient to permit much play of the molecules before they enter into collision, the passage of electricity shows that the "dark space" has shrunk to small dimensions. We naturally infer that the dark space is the mean free path of the molecules of the residual gas, an inference confirmed by experiment.

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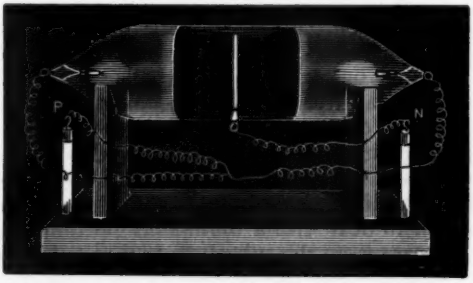

I will endeavor to render this "dark space" visible to all present. Here is a tube (fig. 1), having a pole in the center in the form of a metal disk, and other poles at each end. The center pole is made negative, and the two end poles con-

nected together are made the positive terminal. The dark space will be in the center. When the exhaustion is not very great the dark space extends only a little on each side of the negative pole in the center. When the exhaustion is good, as in the tube before you, and I turn on the coil, the dark space is seen to extend for about an inch on each side of the pole.

Here, then, we see the induction spark actually illuminating the lines of molecular pressure caused by the excitement of the negative pole. The thickness of this dark space is the measure of the mean free path between successive collisions of the molecules of the residual gas. The extra velocity with which the negatively electrified molecules rebound from the excited pole keeps back the more slowly moving molecules which are advancing toward that pole. A conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the discharge.

Therefore the residual gas—or, as I prefer to call it, the gaseous residue—within the dark space is in an entirely different state to that of the residual gas in vessels at a lower degree of exhaustion. To quote the words of our last year's President, in his Address at Dublin:—

"In the exhausted column we have a vehicle for electricity not constant like an ordinary conductor, but itself modified by the passage of the discharge, and perhaps subject to laws differing materially from those which it obeys at atmospheric pressure."

In the vessels with the lower degree of exhaustion, the length of the mean free path of the molecules is exceedingly small as compared with the dimensions of the bulb, and the properties belonging to the ordinary gaseous state of matter, depending upon constant collisions, can be observed. But in the phenomena now about to be examined, so high is the exhaustion carried that the dark space around the negative pole has widened out till it entirely fills the tube. By great rarefaction the mean free path has become so long that the hits in a given time in comparison to the misses may be disregarded and the average molecule is now allowed to obey its own motions or laws without interference. The mean free path, in fact, is comparable to the dimensions of the vessel, and we have no longer to deal with a *continuous* portion of matter, as would be the case were the tubes less highly exhausted, but we must here contemplate the molecules *individually*. In these highly exhausted vessels the molecules of the gaseous residue are able to dart across the tube with comparatively few collisions, and radiating from the pole with enormous velocity, they assume properties so novel and so characteristic as to

entirely justify the application of the term borrowed from Faraday, that of *Radiant Matter.*

Radiant Matter exerts powerful phosphorogenic action where it strikes.

I have mentioned that the Radiant Matter within the dark space excites luminosity where its velocity is arrested by residual gas outside the dark space. But if no residual gas is left, the molecules will have their velocity arrested by the sides of the glass; and here we come to the first and one of the most noteworthy properties of Radiant Matter discharged from the negative pole—its power of exciting phosphorescence when it strikes against solid matter. The number of bodies which respond luminously to this molecular bombardment is very great, and the resulting colors are of every variety. Glass, for instance, is highly phosphorescent when exposed to a stream of Radiant Matter. Here (fig. 2) are three bulbs composed of different glass: one is uranium glass (*a*), which phosphoresces of a dark green color; another is English glass (*b*), which phosphoresces of a blue color; and the third (*c*) is soft German glass,—of which most of the apparatus before you is made,—which phosphoresces of a bright apple-green.

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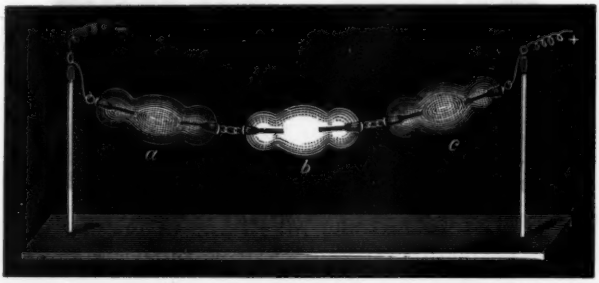

My earlier experiments were almost entirely carried on by the aid of the phosphorescence which glass takes up when it is under the influence of the radiant discharge; but many other substances possess this phosphorescent power in a still higher degree than glass. For instance, here is some of the luminous sulphide of calcium prepared according to M. Ed. Becquerel's description. When the sulphide is exposed to light—even candlelight—it phosphoresces for hours with a bluish white color. It is, however, much more strongly phosphorescent to the molecular discharge in a good vacuum, as you will see when I pass the discharge through this tube.

Other substances besides English, German, and uranium

glass, and Becquerel's luminous sulphides, are also phosphorescent. The rare mineral phenacite (aluminate of glucinum) phosphoresces blue; the mineral spodumene (a silicate of aluminium and lithium) phosphoresces a rich golden yellow; the emerald gives out a crimson light. But without exception, the diamond is the most sensitive substance I have yet met for ready and brilliant phosphorescence. Here is a very curious fluorescent diamond, green by daylight, colorless by candlelight. It is mounted in the center of an exhausted bulb (fig. 3), and the molecular discharge will be directed on it from below upward. On darkening the room you see the diamond shines with as much light as a candle, phosphorescing of a bright green.

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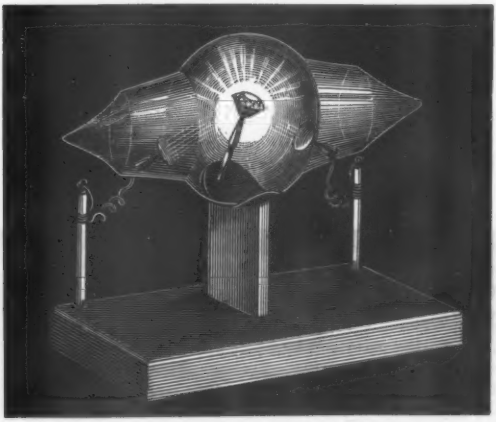

Next to the diamond the ruby is one of the most remarkable stones for phosphorescing. In this tube is a fine collection of ruby pebbles. As soon as the induction spark is turned on you will see these rubies shining with a brilliant rich red tone, as if they were glowing hot. It scarcely matters what color the ruby is, to begin with. In this tube of natural rubies there are stones of all colors—the deep red and also the pale pink ruby. There are some so pale as to be almost colorless, and some of the highly-prized tint of pigeon's blood; but under the impact of Radiant Matter they all phosphoresce with about the same color.

Now the ruby is nothing but crystallized alumina with a little coloring matter. In a paper by Ed. Becquerel,* pub-

* Annales de Chimie et de Physique, 3rd series, vol. lvii, p. 50, 1859.

lished twenty years ago, he describes the appearance of alumina as glowing with a rich red color in the phosphoroscope. Here is some precipitated alumina prepared in the most careful manner. It has been heated to whiteness, and you see it also glows under the molecular discharge with the same rich red color.

The spectrum of the red light emitted by these varieties of alumina is the same as described Becquerel twenty years ago. There is one intense red line, a little below the fixed line B in the spectrum, having a wave-length of about 6895. There is a continuous spectrum beginning at about B, and a few fainter lines beyond it, but they are so faint in comparison with this red line that they may be neglected. This line is easily seen by examining with a small pocket spectroscope the light reflected from a good ruby.

There is one particular degree of exhaustion more favorable than any other for the development of the properties of Radiant Matter which are now under examination. Roughly speaking it may be put at the millionth of an atmosphere.* At this degree of exhaustion the phosphorescence is very strong, and after that it begins to diminish until the spark refuses to pass.†

* 1·0 millionth of an atmosphere = 0·00076 millim.
1315·789 millionths of an atmosphere = 1·0 millim.
1,000,000· " " " = 760·0 millims.
" " " " = 1 atmosphere.

† Nearly 100 years ago Mr. Wm. Morgan communicated to the Royal Society a Paper entitled "Electrical Experiments made to ascertain the Non-conducting Power of a Perfect Vacuum, &c." The following extracts from this Paper, which was published in the Phil. Trans. for 1785 (vol. lxxv, p. 272), will be read with interest:—

"A mercurial gage about fifteen inches long, carefully and accurately boiled till every particle of air was expelled from the inside, was coated with tin-foil five inches down from its sealed end, and being inverted into mercury through a perforation in the brass cap which covered the mouth of the cistern; the whole was cemented together, and the air was exhausted from the inside of the cistern through a valve in the brass cap, which producing a perfect vacuum in the gage formed an instrument peculiarly well adapted for experiments of this kind. Things being thus adjusted (a small wire having been previously fixed on the inside of the cistern to form a communication between the brass cap and the mercury, into which the gage was inverted) the coated end was applied to the conductor of an electrical machine, and notwithstanding every effort, neither the smallest ray of light, nor the slightest charge, could ever be procured in this exhausted gage."

"If the mercury in the gage be imperfectly boiled, the experiment will not succeed; but the color of the electric light, which in air rarefied by an exhauster is always violet or purple, appears in this case of a beautiful green, and, what is very curious, the degree of the air's rarefaction may be nearly determined by this means; for I have known instances, during the course of these experiments, where a small particle of air having found its way into the tube, the electric light became visible, and as usual of a green color; but the charge being often repeated, the gage has at length cracked at its sealed end, and in consequence the external air, by being admitted into the inside, has gradually pro-

I have here a tube (fig. 4) which will serve to illustrate the dependence of the phosphorescence of the glass on the degree of exhaustion. The two poles are at *a* and *b*, and at the end (*c*) is a small supplementary tube connected with the other by

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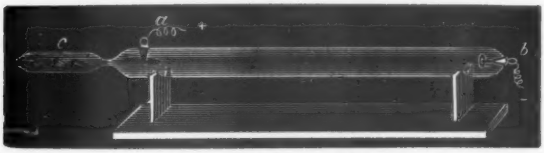

a narrow aperture, and containing solid caustic potash. The tube has been exhausted to a very high point, and the potash heated so as to drive off moisture and injure the vacuum. Exhaustion has then been re-commenced, and the alternate heating and exhaustion repeated until the tube has been brought to the state in which it now appears before you. When the induction spark is first turned on nothing is visible —the vacuum is so high that the tube is non-conducting. I now warm the potash slightly and liberate a trace of aqueous vapor. Instantly conduction commences, and the green phosphorescence flashes out along the length of the tube. I continue the heat, so as to drive off more gas from the potash. The green gets fainter, and now a wave of cloudy luminosity sweeps over the tube, and stratifications appear, which rapidly get narrower, until the spark passes along the tube in the form of a narrow purple line. I take the lamp away, and allow the potash to cool; as it cools, the aqueous vapor, which the heat had driven off, is re-absorbed. The purple line broadens out, and breaks up into fine stratifications; these get wider, and travel toward the potash tube. Now a wave of green light appears on the glass at the other end, sweeping on and driving the last pale stratification into the potash; and now the tube glows over its whole length with the green phosphorescence. I might keep it before you, and show the green growing fainter and the vacuum becoming non-conducting; but I should detain you too long, as time is required for the

duced a change in the electric light from green to blue, from blue to indigo, and so on to violet and purple, till the medium has at length become so dense as no longer to be a conductor of electricity. I think there can be little doubt, from the above experiments, of the non-conducting power of a perfect vacuum."

"This seems to prove that there is a limit even in the rarefaction of air, which sets bounds to its conducting power; or, in other words, that the particles of air may be so far separated from each other as no longer to be able to transmit the electric fluid; that if they are brought within a certain distance of each other, their conducting power begins, and continually increases till their approach also arrives at its limit."

absorption of the last traces of vapor by the potash, and I must pass on to the next subject.

Radiant Matter proceeds in straight lines.

The Radiant Matter, whose impact on the glass causes an evolution of light, absolutely refuses to turn a corner. Here is a V-shaped tube, a pole being at each extremity. The pole at the right side being negative, you see that the whole of the right arm is flooded with green light, but at the bottom it stops sharply and will not turn the corner to get into the left side. When I reverse the current and make the left pole negative, the green changes to the left side, always following the negative pole and leaving the positive side with scarcely any luminosity.

In the ordinary phenomena exhibited by vacuum tubes—phenomena with which we are all familiar—it is customary, in order to bring out the striking contrasts of color, to bend the tubes into very elaborate designs. The luminosity caused by the phosphorescence of the residual gas follows all the convolutions into which skillful glass-blowers can manage to twist the glass. The negative pole being at one end and the positive pole at the other, the luminous phenomena seem to depend more on the positive than on the negative at the ordinary exhaustion hitherto used to get the best phenomena of vacuum tubes. But at a very high exhaustion the phenomena noticed in ordinary vacuum tubes when the induction spark passes through them—an appearance of cloudy luminosity and of stratifications—disappear entirely. No cloud or fog whatever is seen in the body of the tube, and with such a vacuum as I am working with in these experiments, the only light observed is that from the phosphorescent surface of the glass. I have here two bulbs (fig. 5), alike in shape and position of poles, the only difference being that one is at an exhaustion equal to a few millimeters of mercury--such a moderate exhaustion as will give the ordinary luminous phenomena—while the other is exhausted to about the millionth of an atmosphere. I will first connect the moderately exhausted bulb (A) with the induction-coil, and retaining the pole at one side (*a*) always negative, I will put the positive wire successively to the other poles with which the bulb is furnished. You see that as I change the position of the positive pole, the line of violet light joining the two poles changes, the electric current always choosing the shortest path between the two poles, and moving about the bulb as I alter the position of the wires.

This, then, is the kind of phenomenon we get in ordinary exhaustions. I will now try the same experiment with a bulb (B) that is very highly exhausted, and as before, will make

the side pole (a') the negative, the top pole (b) being positive. Notice how widely different is the appearance from that shown by the last bulb. The negative pole is in the form of a shallow cup. The molecular rays from the cup cross in the center

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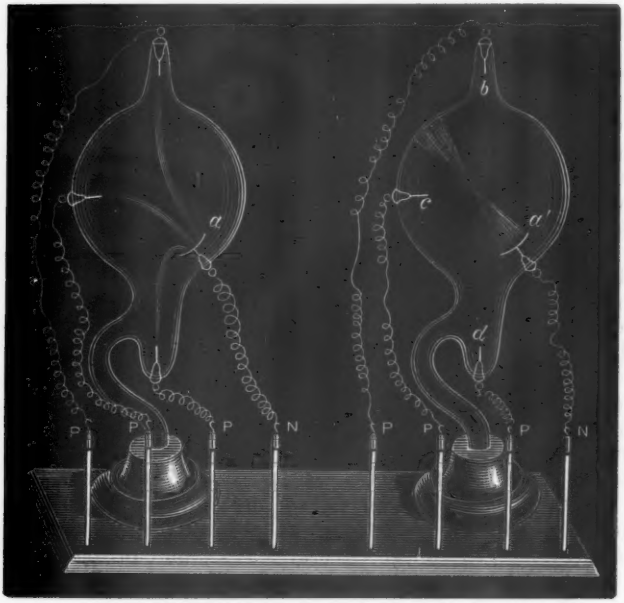

of the bulb, and thence diverging fall on the opposite side and produce a circular patch of green phosphorescent light. As I turn the bulb round you will all be able to see the green patch on the glass. Now observe, I remove the positive wire from the top, and connect it with the side pole (c). The green patch from the divergent negative focus is there still. I now make the lowest pole (d) positive, and the green patch remains where it was at first, unchanged in position or intensity.

We have here another property of Radiant Matter. In the low vacuum the position of the positive pole is of every importance, while in a high vacuum the position of the positive pole scarcely matters at all; the phenomena seem to depend entirely on the negative pole. If the negative pole points in the direction of the positive, all very well, but if the negative pole is entirely in the opposite direction it is of little consequence: the Radiant Matter darts all the same in a straight line from the negative.

If, instead of a flat disk, a hemi-cylinder is used for the negative pole, the Matter still radiates normal to its surface. The (tube before you fig. 6) illustrates this property. It contains, as a negative pole, a hemi-cylinder (*a*) of polished aluminium. This is connected with a fine copper wire, *b*, ending at the platinum terminal, *c*. At the upper end of the tube is another terminal, *d*. The induction-coil is connected so that the hemi-cylinder is negative and the upper pole positive, and when exhausted to a sufficient extent the projection of the molecular rays to a focus is very beautifully shown. The rays of Matter being driven from the hemi-cylinder in a direction normal to its surface, come to a focus and then diverge, tracing their path in brilliant green phosphorescence on the surface of the glass.

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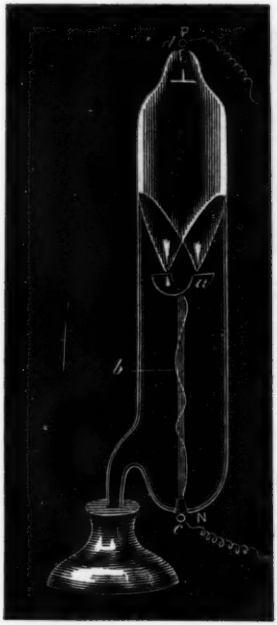

Instead of receiving the molecular rays on the glass, I will show you another tube in which the focus falls on a phosphorescent screen. See how brilliantly the lines of discharge shine out, and how intensely the focal point is illuminated, lighting up the table.

Radiant Matter when intercepted by solid matter casts a shadow.

Radiant Matter comes from the pole in straight lines, and does not merely permeate all the parts of the tube and fill it with light, as would be the case were the exhaustion less good. Where there is nothing in the way the rays strike the screen and produce phosphorescence, and where solid matter intervenes they are obstructed by it, and a shadow is thrown on the screen. In this pear-shaped bulb (fig. 7) the negative pole (*a*) is at the pointed end. In the middle is a cross (*b*) cut out of sheet aluminium, so that the rays from the negative pole projected along the tube will be partly intercepted by the aluminium cross, and will project an image of it on the hemispherical end of the tube which is phosphorescent. I turn on the coil, and you will all see the black shadow of the cross on the luminous end of the bulb (*c*, *d*). Now, the Radiant Matter from the negative pole has been passing by the side of the

aluminium cross to produce the shadow; the glass has been hammered and bombarded till it is appreciably warm, and at

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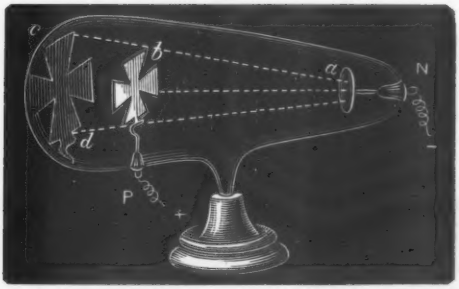

the same time another effect has been produced on the glass—its sensibility has been deadened. The glass has got tired, if I may use the expression, by the enforced phosphorescence. A change has been produced by this molecular bombardment which will prevent the glass from responding easily to additional excitement; but the part that the shadow has fallen on is not tired—it has not been phosphorescing at all and is perfectly fresh; therefore if I throw down this cross,—I can easily do so by giving the apparatus a slight jerk, for it has been most ingeniously constructed with a hinge by Mr. Gimingham,—and so allow the rays from the negative pole to fall uninterruptedly on to the end of the bulb, you will suddenly see the black cross change to a luminous one because the back-ground is now only capable of faintly phosphorescing, while the part which had the black shadow on it retains its full phosphorescent power. The stencilled image of the luminous cross unfortunately soon dies out. After a period of rest the glass partly recovers its power of phosphorescing, but it is never so good as it was at first.

Here, therefore, is another important property of Radiant Matter. It is projected with great velocity from the negative pole, and not only strikes the glass in such a way as to cause it to vibrate and become temporarily luminous while the discharge is going on, but the molecules hammer away with sufficient energy to produce a permanent impression upon the glass.

Radiant Matter exerts strong mechanical action where it strikes.

We have seen from the sharpness of the molecular shadows, that Radiant Matter is arrested by solid matter placed in its path. If this solid body is easily moved the impact of the molecules will reveal itself in strong mechanical action. Mr

Gimingham has constructed for me an ingenious piece of apparatus which when placed in the electric lantern will render this mechanical action visible to all present. It consists of a highly exhausted glass tube (fig. 8), having a little glass railway running along it from one end to the other. The axle of a small wheel revolves on the rails, the spokes of the wheel carrying wide mica paddles. At each end of the tube, and rather above the center, is an aluminium pole, so that whichever pole is made negative the stream of Radiant Matter darts

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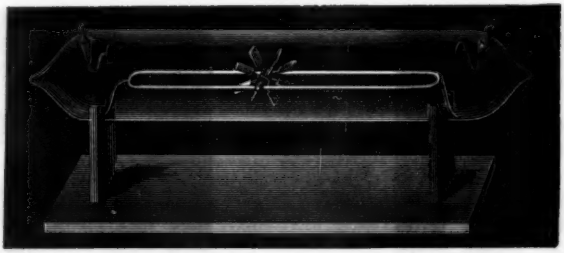

from it along the tube, and striking the upper vanes of the little paddle-wheel causes it to turn round and travel along the railway. By reversing the poles I can arrest the wheel and send it the reverse way, and if I gently incline the tube the force of impact is observed to be sufficient even to drive the wheel up-hill.

This experiment therefore shows that the molecular stream from the negative pole is able to move any light object in front of it.

The molecules being driven violently from the pole there should be a recoil of the pole from the molecules, and by arranging an apparatus so as to have the negative pole movable and the body receiving the impact of the Radiant Matter fixed, this recoil can be rendered sensible. In appearance the apparatus (fig. 9) is not unlike an ordinary radiometer with aluminium disks for vanes, each disk coated on one side with a film of mica. The fly is supported by a hard steel instead of glass cup, and the needle point on which it works is connected by means of a wire with a platinum terminal sealed into the glass. At the top of the radiometer bulb a second terminal is sealed in. The radiometer therefore can be connected with an induction-coil, the movable fly being made the negative pole.

For these mechanical effects the exhaustion need not be so high as when phosphorescence is produced. The best pressure for this electrical radiometer is a little beyond that at which the dark space round the negative pole extends to the

sides of the glass bulb. When the pressure is only a few millims. of mercury, on passing the induction current a halo of velvety violet light forms on the metallic side of the vanes, the mica side remaining dark. As the pressure diminishes, a dark space is seen to separate the violet halo from the metal. At a pressure of half a millimeter this dark space extends to the glass, and rotation commences. On continuing the exhaustion the dark space further widens out and appears to flatten itself against the glass, when the rotation becomes very rapid.

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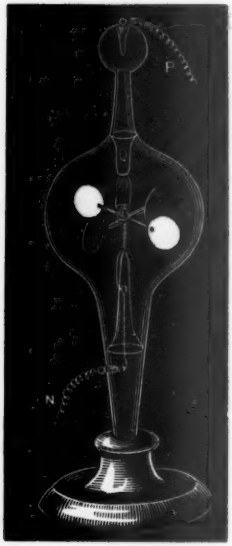

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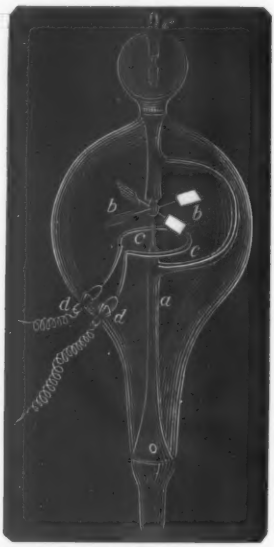


Here is another piece of apparatus (fig. 10) which illustrates the mechanical force of the Radiant Matter from the negative pole. A stem (*a*) carries a needle-point in which revolves a light mica fly (*b b*). The fly consists of four square vanes of thin clear mica, supported on light aluminium arms, and in the center is a small glass cap which rests on the needle-point. The vanes are inclined at an angle of 45° to the horizontal plane. Below the fly is a ring of fine platinum wire (*c c*), the ends of which pass through the glass at *d d*. An aluminium terminal (*e*) is sealed in at the top of the tube, and the whole is exhausted to a very high point.

By means of the electric lantern I project an image of the vanes on the screen. Wires from the induction-coil are attached, so that the platinum ring is made the negative pole,

the aluminium wire (*e*) being positive. Instantly, owing to the projection of Radiant Matter from the platinum ring, the vanes rotate with extreme velocity. Thus far the apparatus has shown nothing more than the previous experiments have prepared us to expect; but observe what now happens. I disconnect the induction-coil altogether, and connect the two ends of the platinum wire with a small galvanic battery; this makes the ring *c c* red-hot, and under this influence you see that the vanes spin as fast as they did when the induction-coil was at work.

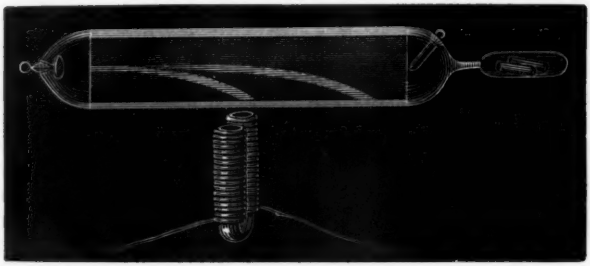
Here, then, is another most important fact. Radiant Matter in these high vacua is not only excited by the negative pole of an induction-coil, but a hot wire will set it in motion with force sufficient to drive round the sloping vanes.

Radiant Matter is deflected by a Magnet.

I now pass to another property of Radiant Matter. This long glass tube is very highly exhausted; it has a negative pole at one end and a long phosphorescent screen down the center of the tube. In front of the negative pole is a plate of mica with a hole in it, and the result is, when I turn on the current, a line of phosphorescent light is projected along the whole length of the tube. I now place beneath the tube a powerful horse-shoe magnet: observe how the line of light becomes curved under the magnetic influence waving about like a flexible wand as I move the magnet to and fro.

This action of the magnet is very curious, and if carefully followed up will elucidate other properties of Radiant Matter. Here (fig. 11) is an exactly similar tube, but having at one end a small potash tube, which if heated will slightly injure the vacuum.

11.



I turn on the induction current, and you see the ray of Radiant Matter tracing its trajectory in a curved line along the screen, under the influence of the horse-shoe magnet beneath. Observe the shape of the curve. The molecules shot from the

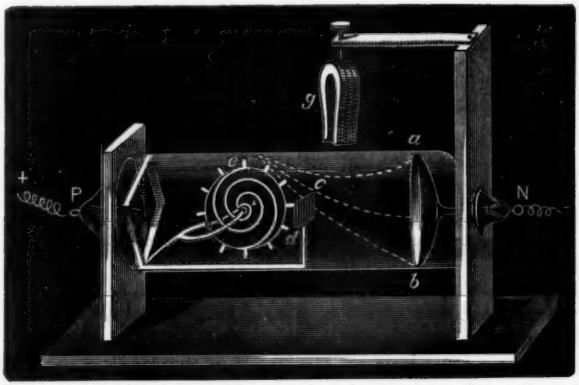
negative pole may be likened to a discharge of iron bullets from a mitrailleuse, and the magnet beneath will represent the earth curving the trajectory of the shot by gravitation. Here on this luminous screen you see the curved trajectory of the shot accurately traced. Now suppose the deflecting force to remain constant, the curve traced by the projectile varies with the velocity. If I put more powder in the gun the velocity will be greater and the trajectory flatter, and if I interpose a denser resisting medium between the gun and the target, I diminish the velocity of the shot, and thereby cause it to move in a greater curve and come to the ground sooner. I cannot well increase before you the velocity of my stream of radiant molecules by putting more powder in my battery, but I will try and make them suffer greater resistance in their flight from one end of the tube to the other. I heat the caustic potash with a spirit-lamp and so throw in a trace more gas. Instantly the stream of Radiant Matter responds. Its velocity is impeded, the magnetism has longer time on which to act on the individual molecules, the trajectory gets more and more curved, until, instead of shooting nearly to the end of the tube, my molecular bullets fall to the bottom before they have got more than half-way.

It is of great interest to ascertain whether the law governing the magnetic deflection of the trajectory of Radiant Matter is the same as has been found to hold good at a lower vacuum. The experiments I have just shown you were with a very high vacuum. Here is a tube with a low vacuum. When I turn on the induction spark, it passes as a narrow line of violet light joining the two poles. Underneath I have a powerful electro-magnet. I make contact with the magnet, and the line of light dips in the center toward the magnet. I reverse the poles, and the line is driven up to the top of the tube. Notice the difference between the two phenomena. Here the action is temporary. The dip takes place under the magnetic influence; the line of discharge then rises and pursues its path to the positive pole. In the high exhaustion, however, after the stream of Radiant Matter had dipped to the magnet it did not recover itself, but continued its path in the altered direction.

By means of this little wheel, skillfully constructed by Mr. Gimingham, I am able to show the magnetic deflection in the electric lantern. The apparatus is shown in this diagram (fig. 12). The negative pole (a, b) is in the form of a very shallow cup. In front of the cup is a mica screen (c, d), wide enough to intercept the Radiant Matter coming from the negative pole. Behind this screen is a mica wheel (e, f) with a series of vanes, making a sort of paddle-wheel. So arranged, the molecular rays from the pole a b will be cut off from the wheel, and will

not produce any movement. I now put a magnet, *g*, over the tube, so as to deflect the stream over or under the obstacle *c d*,

12.



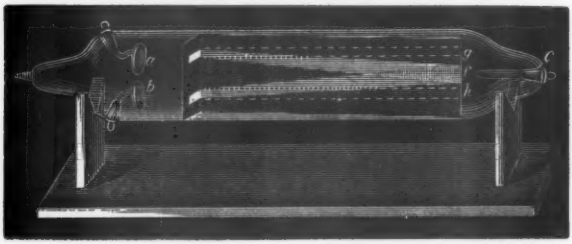
and the result will be rapid motion in one or the other direction, according to the way the magnet is turned. I throw the image of the apparatus on the screen. The spiral lines painted on the wheel show which way it turns. I arrange the magnet to draw the molecular stream so as to beat against the upper vanes, and the wheel revolves rapidly as if it were an over-shot water-wheel. I turn the magnet so as to drive the Radiant Matter underneath; the wheel slackens speed, stops, and then begins to rotate the other way, like an under-shot water-wheel. This can be repeated as often as I reverse the position of the magnet.

I have mentioned that the molecules of the Radiant Matter discharged from the negative pole are negatively electrified. It is probable that their velocity is owing to the mutual repulsion between the similarly electrified pole and the molecules. In less high vacua, such as you saw a few minutes ago, the discharge passes from one pole to another, carrying an electric current, as if it were a flexible wire. Now it is of great interest to ascertain if the stream of Radiant Matter from the negative pole also carries a current. Here (fig. 13) is an apparatus which will decide the question at once. The tube contains two negative terminals (*a*, *b*) close together at one end, and one positive terminal (*c*) at the other. This enables me to send two streams of Radiant Matter side by side along the phosphorescent screen,—or by disconnecting one negative pole, only one stream.

If the streams of Radiant Matter carry an electric current they will act like two parallel conducting wires and attract one

another; but if they are simply built up of negatively electrified molecules they will repel each other.

13.



I will first connect the upper negative pole (*a*) with the coil, and you see the ray shooting along the line *d*, *f*. I now bring the lower negative pole (*b*) into play, and another line (*e*, *h*) darts along the screen. But notice the way the first lien behaves; it jumps up from its first position, *d f*, to *d g*, showing that it is repelled, and if time permitted I could show you that the lower ray is also deflected from its normal direction: therefore the two parallel streams of Radiant Matter exert mutual repulsion, acting not like current carriers, but merely as similarly electrified bodies.

Radiant Matter produces heat when its motion is arrested.

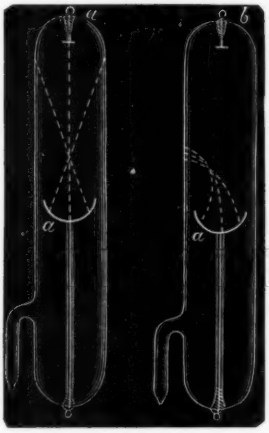
During these experiments another property of Radiant Matter has made itself evident, although I have not yet drawn attention to it. The glass gets very warm where the green phosphorescence is strongest. The molecular focus on the tube, which we saw earlier in the evening (fig. 6) is intensely hot, and I have prepared an apparatus by which this heat at the focus can be rendered apparent to all present.

I have here a small tube (fig. 14, *a*) with a cup-shaped negative pole. This cup projects the rays to a focus in the middle of the tube. At the side of the tube is a small electromagnet, which I can set in action by touching a key, and the focus is then drawn to the side of the glass tube (fig. 14, *b*). To show the first action of the heat I have coated the tube with wax. I will put the apparatus in front of the electric lantern, and throw a magnified image of the tube on the screen. The coil is now at work, and the focus of molecular rays is projected along the tube. I turn the magnetism on, and draw the focus to the side of the glass. The first thing you see is a small circular patch melted in the coating of wax. The glass soon begins to disintegrate, and cracks are shooting starwise from the center of heat. The glass is softening. Now the

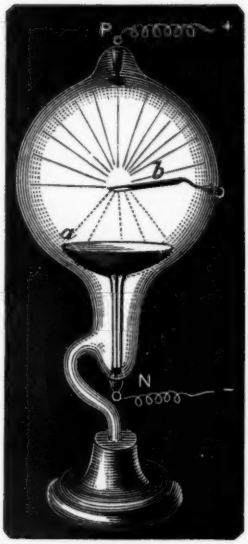
atmospheric pressure forces it in, and now it melts. A hole is perforated in the middle, the air rushes in, and the experiment is at an end.

I can render this focal heat more evident if I allow it to play on a piece of metal. This bulb (fig. 15) is furnished with a

14.



15.



negative pole in the form of a cup (*a*). The rays will therefore be projected to a focus on a piece of iridio-platinum (*b*) supported in the center of the bulb.

I first turn on the induction-coil slightly, so as not to bring out its full power. The focus is now playing on the metal, raising it to a white-heat. I bring a small magnet near, and you see I can deflect the focus of heat just as I did the luminous focus in the other tube. By shifting the magnet I can drive the focus up and down, or draw it completely away from the metal, and leave it non-luminous. I withdraw the magnet, and let the molecules have full play again; the metal is now white-hot. I increase the intensity of the spark. The iridio-platinum glows with almost insupportable brilliancy, and at last melts.

The Chemistry of Radiant Matter.

As might be expected, the chemical distinctions between one kind of Radiant Matter and another at these high exhaustions are difficult to recognize. The physical properties I have been elucidating seem to be common to all matter at this low density.

Whether the gas originally under experiment be hydrogen, carbonic acid, or atmospheric air, the phenomena of phosphorescence, shadows, magnetic deflection, etc., are identical, only they commence at different pressures. Other facts however, show that at this low density the molecules retain their chemical characteristics. Thus by introducing into the tubes appropriate absorbents of residual gas, I can see that chemical attraction goes on long after the attenuation has reached the best stage for showing the phenomena now under illustration, and I am able by this means to carry the exhaustion to much higher degrees than I can get by mere pumping. Working with aqueous vapor I can use phosphoric anhydride as an absorbent; with carbonic acid, potash; with hydrogen, palladium; and with oxygen, carbon, and then potash. The highest vacuum I have yet succeeded in obtaining has been the 1-20,000,000th of an atmosphere, a degree which may be better understood if I say that it corresponds to about the hundredth of an inch in a barometric column three miles high.

It may be objected that it is hardly consistent to attach primary importance to the presence of *Matter*, when I have taken extraordinary pains to remove as much Matter as possible from these bulbs and these tubes, and have succeeded so far as to leave only about the one-millionth of an atmosphere in them. At its ordinary pressure the atmosphere is not very dense, and its recognition as a constituent of the world of Matter is quite a modern notion. It would seem that when divided by a million, so little Matter will necessarily be left that we may justifiably neglect the trifling residue and apply the term *vacuum* to space from which the air has been so nearly removed. To do so, however, would be a great error, attributable to our limited faculties being unable to grasp high numbers. It is generally taken for granted that when a number is divided by a million the quotient must necessarily be small, whereas it may happen that the original number is so large that its division by a million seems to make little impression on it. According to the best authorities, a bulb of the size of the one before you (13·5 centimeters in diameter) contains more than 1,000000,000000,000000,000000 (a quadrillion) molecules. Now, when exhausted to a millionth of an atmosphere we shall still have a trillion molecules left in the bulb—a number quite sufficient to justify me in speaking of the residue as *Matter*.

To suggest some idea of this vast number I take the exhausted bulb, and perforate it by a spark from the induction coil. The spark produces a hole of microscopical fineness, yet sufficient to allow molecules to penetrate and to destroy the

vacuum. The inrush of air impinges against the vanes and sets them rotating after the manner of a windmill. Let us suppose the molecules to be of such a size that at every second of time a hundred millions could enter. How long, think you, would it take for this small vessel to get full of air? An hour? A day? A year? A century? Nay, almost an eternity! A time so enormous that imagination itself cannot grasp the reality. Supposing this exhausted glass bulb, indued with indestructibility, had been pierced at the birth of the solar system; supposing it to have been present when the earth was without form and void; supposing it to have borne witness to all the stupendous changes evolved during the full cycles of geologic time, to have seen the first living creature appear, and the last man disappear; supposing it to survive until the fulfilment of the mathematicians' prediction that the sun, the source of energy, four million centuries from its formation will ultimately become a burnt-out cinder;* supposing all this,—at the rate of filling I have just described, 100 million molecules a second—this little bulb even then would scarcely have admitted its full quadrillion of molecules.†

But what will you say if I tell you that all these molecules, this quadrillion of molecules, will enter through the microscopic hole before you leave this room? The hole being unaltered in size, the number of molecules undiminished, this apparent paradox can only be explained by again supposing the size of the molecules to be diminished almost infinitely—so that instead of entering at the rate of 100 millions every second, they troop in at a rate of something like 300 millions a second. I have done the sum, but figures when they mount so high cease to have any meaning, and such calculations are as futile as trying to count the drops in the ocean.

In studying this Fourth state of Matter we seem at length to have within our grasp and obedient to our control the little indivisible particles which with good warrant are supposed to

* The possible duration of the sun from formation to extinction has been variously estimated by different authorities, at from 18 million years to 400 million years. For the purpose of this illustration I have taken the highest estimate.

† According to Mr. Johnstone Stoney (Phil. Mag., vol. xxxvi. p. 141), 1 c.c. of air contains about 1000,000000,000000,000000 molecules. Therefore a bulb 13·5 centimeters diameter contains $13{\cdot}5^3 \times 0{\cdot}5236 \times 1000,000000,000000,000000$ or 1,288252,350000,000000,000000 molecules of air at the ordinary pressure. Therefore the bulb when exhausted to the millionth of an atmosphere contains 1,288252,350000,000000 molecules, leaving 1,288251,061747,650000,000000 molecules to enter through the perforation. At the rate of 100,000000 molecules a second, the time required for them all to enter will be

12882,510617,476500 seconds, or
214,708510,291275 minutes, or
3,578475,171521 hours, or
149103,132147 days, or
408,501731 years.

constitute the physical basis of the universe. We have seen that in some of its properties Radiant Matter is as material as this table, whilst in other properties it almost assumes the character of Radiant Energy. We have actually touched the border land where Matter and Force seem to merge into one another, the shadowy realm between Known and Unknown which for me has always had peculiar temptations. I venture to think that the greatest scientific problems of the future will find their solution in this Border Land, and even beyond; here, it seems to me, lie Ultimate Realities, subtle, far-reaching, wonderful.

"Yet all these were, when no Man did them know,
Yet have from wisest Ages hidden beene;
And later Times thinges more unknowne shall show.
Why then should witlesse Man so much misweene,
That nothing is, but that which he hath seene?"

ART. XXXVI.—*On the Coincidence of the Bright Lines of the Oxygen Spectrum with Bright Lines in the Solar Spectrum;* by HENRY DRAPER, M.D.*

I INTEND in this paper to speak of the steps that led to the discovery of oxygen in the Sun, to describe very briefly some of the successive improvements of the electrical and optical apparatus employed, and finally to discuss the earlier results and to show their subsequent confirmation.

In 1857, after the meeting of the British Association at Dublin, some of the members, by the kindness of the Earl of Rosse, were invited to visit the 6-foot Reflector at Birr Castle. In this way I enjoyed the advantage of seeing the methods by which that great instrument had been produced, and, on returning to America in 1858, it prompted me to begin the construction of a metallic speculum of 15½ inches aperture. Soon after, by the advice of Sir John Herschel, who had early information of Foucault's work in Paris, the metal was abandoned in favor of silvered glass, and several mirrors were ground and polished. The telescope was constructed especially for photography, and good results were obtained in 1863, culminating in the production of a photograph of the Moon fifty inches in diameter. These were published in the Smithsonian Contributions to Science for the succeeding year. The success procured with this instrument prepared the way for making a silvered glass Equatoreal of twenty-eight inches aperture, which was ready for use

*Read before the Royal Astronomical Society, June 13th, 1879, and reprinted from advance sheets of the Monthly Notices. This Journal is indebted for the cuts illustrating this article, to the Astronomical Society.

in 1871, though it has been much modified since. It was obvious that increased light-collecting power and precise equatoreal movements were necessary for the modern applications of physics to astronomy. More recently still there has been attached to the same equatoreal stand an achromatic telescope of twelve inches aperture made by Alvan Clark & Sons, this being particularly intended for solar spectroscopic work.

Soon after the 28-inch Reflector was turned to stellar and planetary photographic spectroscopy it became evident that the results obtained required for their interpretation photographs of metallic and non-metallic spectra, so that comparisons might be instituted leading to precise knowledge of the elements producing lines at the more refrangible end of the spectrum. This led to a division of the work into two parts, one for the Observatory in the country in the warmer half of the year, the other for my town laboratory during the winter. It was in the latter that most of the oxygen work has been done, and consequently the engine, the Gramme machine, the induction coil, and the large spectroscope are generally there.

My first photographs of metallic spectra were taken with such apparatus as happened to be at hand, viz: a couple of Bunsen's batteries, an induction coil giving a spark of one-half inch, and a Hofmann's direct-vision spectroscope. The length of the spectrum from G to H was about half an inch, but, though the dimensions were small, the promise was great. After some experiments, however, and after obtaining more powerful instrumental appliances, it seemed best, as able physicists were engaged on the metallic spectra, to turn attention more particularly to photographing the spectra of the non-metals. The exceedingly valuable researches of Dr. Huggins had brought the astronomical importance of nitrogen, carbon and hydrogen into notice, and these accordingly were next the subject of experiment. Not long after, on examining a series of photographs of the fluted spectrum of nitrogen taken with juxtaposed solar spectra, the suspicion that there was a coincidence of some bright bands in the two spectra was suggested. On pursuing the subject with more and more powerful electrical and optical arrangements, the coincidence of bright lines of oxygen with bright lines in the solar spectrum was discovered.

The original apparatus, as has been said above, was on a very small scale, but it was soon replaced by a larger battery, a 2-inch induction coil, and a direct-vision prism of one inch aperture by Browning. The electrical part was made more and more powerful as the research proceeded, the 2-inch induction coil being succeeded by one of six inches, and that in turn by a Ruhmkorff coil capable of giving a spark of seventeen inches. The battery was eventually superseded by a Gramme dynamo-

electric machine which can produce a current powerful enough to give, between carbon points, a light equal to 500 standard candles. When this machine is properly applied to the 17-inch induction coil, it will readily give 1,000 10-inch sparks per minute. These, being condensed by fourteen Leyden jars, communicate an intense incandescence to air, and light enough is produced to permit of the use of a narrow slit, and of a collimator and telescope of long focus.

Since 1877, when the first publication of the discovery of oxygen in the Sun was made, still further improvements, especially in the optical parts, have been completed, so that I am now enabled to photograph the oxygen spectrum with four times the dispersion then employed. For the sake of clearness, it is best to give a brief description: 1st, of the electrical part; and 2nd, of the optical part.

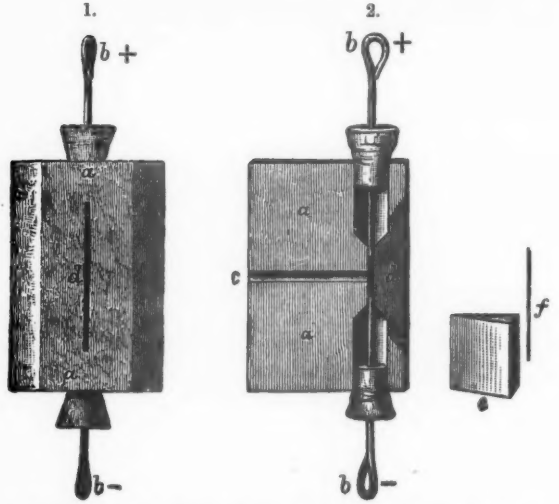
The electrical part consists of the Gramme machine and its driving engine, the induction coil, the Leyden jars, and the terminal or spark compressor. An advantage the Gramme has over a battery is in the uniformity of the current it gives when an uniform rate of rotation of its bobbin is kept up. Of course this implies the use of a prime mover that is well regulated. The petroleum engine of one and a half horse-power I have employed is convenient and safe and does this duty well. As to the Gramme itself, it is only needful to call attention to a modification of the interior connections. In one form the bobbin of wire which revolves between the magnets is double, so that the current produced may be divided into two. Under ordinary circumstances, where the machine is used to produce light, both sides of the bobbin send their currents through the electro-magnets. But if the whole current be sent through a quick-working break circuit into an induction coil, the electro-magnets do not become sufficiently magnetised to produce any appreciable effect. It is expedient, therefore, to arrange the connections so that one-half of the bobbin gives a continuous current through the electro-magnets and keeps up the intensity of the magnetic field, and then the current from the other half of the bobbin may be used for exterior work, whether continuous or interrupted.

At first a Foucault mercurial interruptor was arranged to make and break the current passing into the primary circuit of the induction coil; but during the past year, by carrying the rate of rotation of the Gramme up to 1,000 per minute, the strength of the current has been so much increased that the mercury was driven violently out of the cup, and hence it was essential to arrange a mechanical break in which solid metal alone was used. This has been accomplished by fastening on the axis of the Gramme bobbin a wheel with an interrupted rim, which serves the purpose well.

As to the induction coil, it is only needful to say that it gives a good thick spark, which is limited to twelve inches to avoid the risk of injuring the insulation. The Leyden jars are fourteen in number, having altogether seven square feet of coating on each surface.

The arrangement of the terminals from the Leyden jars to get the steadiest and brightest effect has offered great difficulties. The condensed spark taken in the open air or in a gas under atmospheric pressure pursues, if unconfined, a zigzag course, and this is apt to produce a widening of the lines in the photographed spectrum. But, after many experiments, it turned out that the spark might be compressed between two plates of thick glass, or, better yet, between two plates of soapstone. If the interval between the plates was directed toward the slit of the spectroscope the lateral flickering of the spark was prevented, and yet at the same time the spark was freely exposed to the slit without the intervention of glass or any substance on which the volatilized metal from the terminals could deposit. Very early in this research it had become apparent that Plücker's tubes could not be employed with electrical currents of more than a certain intensity, partly on account of the deposit that took place in the capillary portion, and partly because the terminals became so hot as to melt and crack the glass. Moreover, it was desirable to use one terminal of iron, so as to be sure that the spectrum of the gas was correctly adjusted to the solar spectrum, and this is impracticable with Plücker's tubes. An additional advantage arises from the soapstone plates, viz: the temperature of the small volume of air between the terminals is materially increased, and increased brightness results. I have tried the effect of warming the air by passing it through a coil of brass tube maintained at a bright red heat, but this does not seem to make any perceptible difference when the terminals are enclosed in the spark compressor.

The optical part of my apparatus has undergone many modifications. At first a Hofmann direct-vision prism was combined with a lens of six inches focus; this was soon after replaced by a Browning direct-vision prism and a lens of eighteen inches focus, the latter being arranged for conjugate foci, so that it was virtually as if collimating and observing lenses of thirty-six inches focus were employed. The final system, perfected this winter, consists of a collimator of two inches aperture and twenty-six inches focus, succeeded by two bisulphide of carbon prisms of two inches aperture and an observing or photographing lens of six feet six inches focal length. These prisms belong to Mr. Rutherfurd and are the same he made for producing his celebrated solar prismatic spectrum. This gives a dispersion of about eight inches between G and H and enables



SPARK COMPRESSOR: 1, front view; 2, section in plane of narrow opening; *aa*, soapstone; *bb*, terminals; *c*, aperture for introducing gases; *d*, narrow opening to spark; *e*, right-angled prism; *f*, slit of spectroscope.

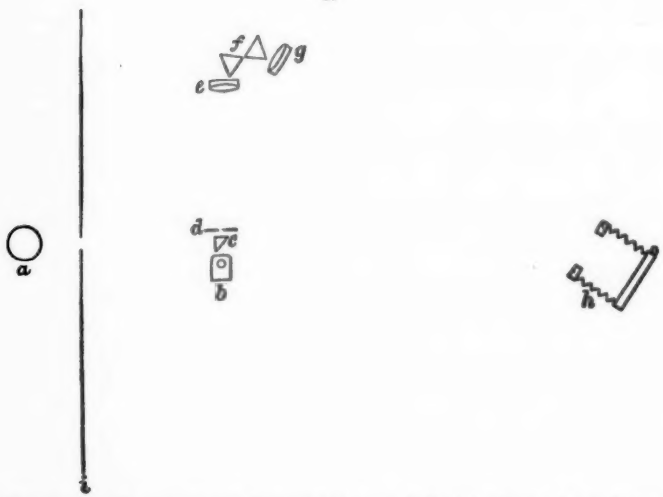


DIAGRAM OF PHOTOGRAPHIC SPECTROSCOPE: *a*, heliostat mirror; *b*, spark compressor; *c*, right-angled prism; *d*, slit; *e*, collimator; *f*, two bisulphide prisms; *g*, photographic objective; *h*, camera; *i*, window shutter.

me to get original negatives on a scale about half the size of Ångström's charts in the Spectre Normal du Soleil. When it is remembered that the light produced by the electric current in the spark compressor is scarcely equal to one standard candle, it will be realized that this great dispersion nearly attains the limit of present possibility. By comparison I have found, when the electric arc from this Gramme volatilizes iron, the light is sixty times stronger than the most vivid incandescence of air that I have produced.

The slit of the spectroscope is about one inch long, and opposite the lower half is placed a right-angled prism which serves to bring in a beam of sunlight from a heliostat. We thus have the solar spectrum and the air spectrum upon the plate at the same time, so that the two spectra on the negative are, strictly speaking, simultaneously produced. Moreover, by the aid of a magnifier we can ascertain, just previous to an exposure, whether the adjustments are in the best order. It is not commonly known that, to obtain the last degree of exactness in coincidence between a solar and an air spectrum, many precautions are necessary, and that is the reason it is desirable to have iron vapor present at one of the poles so as to determine the reliability of the coincidence by comparing iron in the spark spectrum with iron in the Sun.

Having thus alluded to some of the principal peculiarities of the apparatus constructed for this research, it is proper in the next place to point out the nature of the evidence afforded by the photographs of the presence of oxygen in the Sun. The first photographs were on so small a scale that they did not even give rise to a suspicion of this fact, and it was not until 1876 that I felt sufficiently sure to make any publication. At this time the original negatives were about two inches long from G to H, and they bore an enlargement of three or four times quite well. The Albertype printed in 1877 in Nature, the Comptes Rendus, and this Journal, was produced from such an enlargement. Since that time, in order to meet the criticism that perhaps the dispersion was not sufficient to disclose the lack of coincidence if such existed, I have increased the dispersion four times and am thus enabled to make enlarged photographs on a scale about twice the size of Ångström's chart. Enlargements of the juxtaposed spectra of air and Sun on this scale are now presented for inspection.

Of course an enlargement never does justice to the original from which it was produced, and, in order to study the matter faithfully, the negative must be examined carefully with a magnifier. Beside this, owing partly to the fact that the solar spectrum has suffered from absorptive influences, both in the Earth's atmosphere and in the solar atmosphere, the conditions

under which the oxygen spectrum is seen when compared with the spark spectrum are modified. In fact, a critical study of the two spectra demands that each line of oxygen should be separately photographed with the corresponding region of the Sun's spectrum, so as to reproduce as nearly as possible the same conditions for each. As an instance of the modifications which may be caused by the solar atmosphere, the superposition of absorption lines on the bright lines of oxygen may be mentioned. If, as seems to be the case, the stratum giving the oxygen spectrum in the Sun lies deeper than the reversing layer in which iron exists, I see no reason why an iron absorption line, for instance, may not fall upon an oxygen bright band. In support of this supposition that oxygen is photospheric, it may be stated that, though I have examined the chromosphere on many occasions, I have not as yet seen the bright oxygen lines project beyond the apparent limb of the Sun as observed in the spectroscope, although several of the chromosphere lines catalogued by Young were readily visible. On consulting with Professor Young, he expressed the opinion that the oxygen groups near G did not appear as bright lines in the chromosphere, even under the exceptionally favorable circumstances he enjoyed at Sherman. For the purpose of continuing the study of this point, and also of examining small areas on the Sun, faculæ, spots, etc., Alvan Clark & Sons are constructing a special spectroscope for me which can give the dispersion of twenty heavy flint prisms and can bear high magnifying power.

If it be conceded that there are bright lines in the spectrum of the solar disk, which seems to be the opinion of several physicists, and especially Lockyer, Cornu and Hennessy, the question of their origin naturally attracts attention. It seems that there is great probability, from general chemical reasons, that a number of the non-metals may exist in the Sun. The obvious continuation of this research is in that direction. But the subject is surrounded by exceedingly great obstacles, arising principally from the difficulty of matching the conditions as to temperature, pressure, etc., found in the Sun. Any one who has studied nitrogen, sulphur, or carbon, and has observed the manner in which the spectrum changes by variations of heat and pressure, will realize that it is well-nigh impossible to hit upon the exact conditions under which such bodies exist at the level of the photosphere. The fact that oxygen, within a certain range of variation, suffers less change than others of the non-metals has been the secret of its detection in the Sun. It appears to have a greater stability of constitution, though Schuster has shown that its spectrum may be made to vary. I have already begun an extended series of experiments on the non-metals; but the results exhibit such confusion that their

bearing cannot at present be distinctly seen. In the case of nitrogen the broad bands between G and H exhibit, under the most intense incandescence, a tendency to condense into narrow bands or lines, and indeed there are some sharp lines of nitrogen in the photographs now presented.

It does not follow, therefore, that the bright bands of oxygen are necessarily the brightest parts of the solar spectrum. Other substances may produce lines or bands of greater brilliancy.

There is also another cause for a difference of appearance in a bright-line spectrum produced in a laboratory and bright lines in the Sun. While the edges of a band in the spark spectrum may be nebulous or shaded off, the corresponding band in the solar spectrum may have its edges sharpened by the action of adjacent dark lines due to one or another of the metallic substances in the Sun.

On the whole, it does not seem improper for me to take the ground that, having shown by photographs that the bright lines of the oxygen spark spectrum all fall opposite bright portions of the solar spectrum, I have established the probability of the existence of oxygen in the Sun. Causes that can modify in some measure the character of the bright bands of the solar spectrum obviously exist in the Sun, and these, it may be inferred, exert influence enough to account for such minor differences as may be detected.

In closing, it may be well to give some idea of the amount of labor and time this research has already consumed, and this cannot be better done than by a statement of the production of electrical action that has been necessary. Each photograph demands an exposure of 15 minutes, and, with preparation and development, at least half an hour is needed. The making of a photograph, exclusive of intermediate trials, requires, therefore, about 30,000 10-inch sparks, that is 30,000 revolutions of the bobbin of the Gramme machine. In the last three years the Gramme has made 20 millions of revolutions. The petroleum engine only consumes a couple of drops of oil at each stroke, and yet it has used up about 150 gallons. Each drop of oil produces two or three 10-inch sparks. It must also be borne in mind that comparison spectra can only be made when the Sun is shining, and clouds therefore are a fertile source of loss of time.

Appendix.

[We find in the Astronomical Register a Report of the Discussion which followed the reading of Dr. Draper's paper. As this is the expression of the opinion of the best English authorities upon the conclusions reached in the memoir and as it will be seen, we suppose, by but few of our readers, we present it in pretty full abstract, as a matter of general interest.—Eds.]

After the reading of the paper, Dr. Draper showed some exquisitely sharp negatives of the solar and oxygen spectra, which he had obtained, and handed round some paper enlargements, some two feet long, for inspection by the meeting.

Mr. Ranyard: After the reception that has been given to Dr. Draper, I do not think I need say anything about the importance of the research he has undertaken. When a couple of years ago he sent over copies from his former photographs on a much smaller scale, I then ventured to say that I thought the probability of the proposition which he laid down was very great indeed, amounting to some thousands to one, and I should like now to point out how enormously the probability has been increased by these more recent experiments. If Dr. Draper has increased his dispersion four times he has not merely increased the probability of his case four times, but he has increased the value of every coincidence he shows four times. On looking at the original photographs (which show the coincidences more sharply than the paper prints) I counted eighteen oxygen lines, and, therefore, the increase of probability on the present occasion, as compared with the former occasion, is as four to the power of eighteen to one, a very enormous number. There are two or three ways of looking at the probability of the proposition which Dr. Draper has laid down, that the bright lines of oxygen coincide with the bright lines in the solar spectrum. In the first place, there is the chance that the center of no single line of oxygen should fall opposite to a dark line or space in the solar spectrum. If a line were thrown at random beside the solar spectrum the chance that it should fall opposite to a bright part of the solar spectrum would be as the total breadth of the bright part of the solar spectrum to the total breadth of the dark part of the solar spectrum. Let us suppose that the breadth occupied by dark lines in the solar spectrum is equal to the breadth occupied by bright lines (probably as seen with high dispersion this is well within the truth) then the chance of any oxygen line falling opposite to a bright line or interspace in the solar spectrum would be one-half, and if the eighteen lines of oxygen had been thrown down at random beside the solar spectrum, the chance that the center of all the lines should fall opposite to bright spaces would be one divided by two to the power of eighteen. In addition to this we must take into account the chances of the centers of the bright lines appearing to coincide with the centers of the bright interspaces where they do coincide, for in some instances the interspaces are double the breadth of the corresponding oxygen line, the oxygen line being on one side and coinciding with one edge of the interspace; but this is accounted for by the fact that there are probably other bright lines in the solar spectrum besides oxy-

gen lines, and two such bright lines happen, in some instances to fall together. Then, also, it must be remembered that the light of the bright lines of the solar spectrum has suffered absorption in the solar atmosphere, and in the earth's atmosphere, so that they may be modified by the superposition of absorption lines. The observations of the bright lines seen at the limb of the Sun renders it probable that the layer of the solar atmosphere, which gives rise to the dark lines, lies above the layer from which we receive the light of the bright oxygen spectrum, for no bright oxygen lines are seen in the chromosphere. So that it is possible to conceive that the oxygen lines of the solar spectrum may be modified as we observe them by the superposition of dark lines, and this appears in one or two instances to have been the case; but the character of the bright interspaces is very little changed. If, after examining the photographs, anybody has still any doubts as to the case made out by Dr. Draper, I think that he will feel obliged to them if they will state their objections, and give him an opportunity of discussing them.

Mr. Christie: I do not feel exactly competent to enter on this question very deeply, but Dr. Draper has very kindly given me an opportunity of examining his very beautiful photographs, and I cannot help expressing my great admiration at the splendid results he has achieved. He has obtained photographs of the spectrum of oxygen which, as far as my rather limited acquaintance with the subject goes, are far superior to anything else of the kind; but that is quite another question from proving the existence of oxygen in the Sun. I am afraid that is a very difficult question he has undertaken. If he could prove it, I think these photographs would go a very long way. I do not see myself that there is, as he has expressed it, very much more to be done in the matter. But I am sorry to say it seems to open up to me rather a hopeless prospect, because we are brought face to face with a great difficulty. Dr. Draper was very kind in explaining to me his views, and he did not at all shirk the difficulties of the case. I hope I may be justified in alluding to them, simply for the sake of getting at the truth. According to Dr. Draper's supposition, the solar spectrum is made up of a continuous spectrum with dark absorption lines, and also bright lines superposed upon it. Now, the ordinary spectrum, which up to the present time we have supposed that we had to deal with, was a continuous spectrum with dark lines, but when you superpose on this bright lines, so faint that you cannot distinguish their brightness from the general background of the spectrum, it is evident that the problem becomes more complicated. I do not want to express any opinion one way or the other. I am in the position of a sceptic; it may be

an unfortunate position, but I think it is a truly scientific position. Now, taking the position of a sceptic, one has to examine these photographs and look to whether Dr. Draper has shown the coincidences as Mr. Ranyard asserts; if not, his probabilities fall to the ground. In more than one instance I find an oxygen line opposite a broader bright space in the solar spectrum, which appears of identically the same brightness in its whole breadth. If the broader space consists of two or more bright lines, as has been suggested, we have two difficulties to contend with; in the first place we have to show that there are other substances which would give lines corresponding to the unoccupied breadth of the interspace, and in the second place we have the fact that these lines are undistinguishable in brightness from the oxygen lines. Again, each of the oxygen lines is fuzzy at the edges, and as there is nothing analogous in the solar spectrum, we have to suppose that the fuzzy edges are cut off by adjacent dark lines. Now, if we are to make use of probabilities, I would ask what is the probability of a pair of dark lines falling in every case exactly at the edges of an oxygen line? We must also take into account the fact that former physicists have failed to identify any of the bright lines of oxygen with dark lines in the solar spectrum, and therefore we start with the fact that none of the oxygen lines can fall opposite to dark lines.

Mr. Proctor said: There are one or two points I should like to mention. I do not think that Mr. Christie has sufficiently taken into account the importance of the fact that all the oxygen lines fall entirely opposite to bright interspaces in the solar spectrum, and that none of them even partially overlap dark lines. With regard to the general character of the solar spectrum, I would like to ask those who have more especially studied the subject, whether it does not seem probable from antecedent considerations that the solar spectrum should be purely gaseous in its character, that is to say, that there should be bright and dark lines, but no continuous background of spectrum. Perhaps some of you have considered the bearing of Mr. Croll's researches on this matter. He considers that the earth affords evidence that the solar system has been in existence for more than 20,000,000 of years, whereas, if the Sun has been giving out energy as at present for 20,000,000 years, we know that it cannot have derived it from shrinking from even an infinite volume to a globe as large as the photosphere. I am not inclined to agree with him in his explanation that the solar energy may in part have been derived from the impact of stars, but rather think that it points to the fact that the real mass of the Sun lies very much within the photosphere, and that if there is any solid or liquid nucleus, it is only at a depth of many thousands of miles below the photosphere. If that is

the case, I would ask whether it is antecedently probable that there should be any continuous background in the solar spectrum. If the photosphere is purely gaseous, we should only have bright bands interfered with and modified by absorption lines.

Dr. Gladstone: I have listened with the greatest attention to Dr. Draper. When the photographs first came over I was not convinced, but certainly he has produced results, which, as Mr. Ranyard has shown, have largely increased the evidence of there being real coincidences between the oxygen lines and bright spaces in the solar spectrum. There seems to be still a great question as to whether the solar spectrum is made up only of bright and dark lines, or whether there is a background of continuous spectrum. I am not disposed to give up the idea that we have a continuous spectrum underlying these dark lines, but think that it is certain that we have also bright lines mixed with the dark. We know that when we look at the edge of the Sun there are bright lines corresponding to hydrogen, and some other elements to be seen, but there are no oxygen lines. Now, I would suggest that this shows that the oxygen never rises to the level of the chromosphere, so as to be seen at the limb of the Sun, and probably that is just the reason why we see its lines as bright lines and not as dark lines, for it never gets up to a level where it is sufficiently cool to form dark lines. We can easily understand that, with so much iron and magnesium vapor, all the oxygen as it rushes upward to the higher levels may enter into combination and fall in a rain of oxides.

Dr. Huggins said that he had examined Dr. Draper's photographs, and was overwhelmed with a sense of the large amount of conscientious labor and care which he had evidently bestowed upon the investigation. Dr. Draper had made out a *primâ facie* case, which entitled him to demand a careful examination of the evidence he had brought forward; but for his own part he should like to suspend his judgment until he had had an opportunity to reëxamine that part of the spectrum. He preferred to wait for a little light, a little sunlight, on the subject, but he wished now to state how thoroughly impressed he was with the cautious and careful experimental arrangements which Dr. Draper had adopted.

Capt. Noble: It seems to me, looking at the photographs impartially, that if we are to deny the evidence supplied by some of these coincidences, and notably by this group of four lines, and accept Mr. Christie's dicta, we literally should have no tangible evidence as to the existence of any element in the Sun at all.

Mr. Ranyard: I should like to refer to one or two of the objections raised by Mr. Christie. I understood him to urge as

an objection that there are a great many bright spaces which have not been matched with the bright lines of non-metallic elements. But I would remind him that there are also many dark lines which have not been matched with the lines of known elements. Perhaps they will never be matched, for the conditions in the Sun may not correspond to the conditions obtainable in our laboratories. With regard to the bright lines falling opposite interspaces which are broader than the bright lines of oxygen, the probability is, as Mr. Christie states, very great against their being two adjacent bright lines of exactly equal brightness, but it must be remembered that we are not examining the bright lines themselves, but only photographs of the lines, and that the bright parts of these photographs are what would be called by photographers over-exposed, and, consequently, the gradations of brightness are very much obliterated. Again, I understood Mr. Christie to say that if, instead of matching oxygen lines with interspaces, you began the other way, you ought to find the brightest lines of the spectrum matching the oxygen lines, but this involves the assumption that oxygen gives brighter lines than any other element in the Sun. But I would ask what reason we have for assuming that oxygen gives brighter lines than any other element? With regard to the probability of there being a continuous background of brightness in the solar spectrum, it should be remembered that there are theoretical considerations which render it probable that there is always such a continuous background more or less faint between the bright lines in the spectrum of a gas—when the pressure is considerable the brightness of the continuous part of the spectrum is conspicuous; but theoretically there will always be a continuous spectrum corresponding to the short interval of time after the impact of molecules during which the jar of the collision lasts. In the free path between the impacts they give out the characteristic wave lengths, but however rare the gas I suppose there will always be some continuous spectrum corresponding to the impacts, and with the spectra of a great many gaseous elements superposed the continuous spectrum may be conspicuous. We need not, therefore, assume if we should find evidence of such a continuous spectrum that there is any solid or liquid matter in the photosphere; and if it should turn out that there are no oxygen lines above the photosphere, it will not, I think, follow that there is no oxygen there, for there seems to be evidence that the spectrum of some of the elements changes materially at different altitudes, for example, the 1474 line is the brightest of the bright lines in the corona, but it is a very faint line at the level of the reversing layer, and D_3 is a very bright line in the lower chromosphere, but there is no dark line corresponding to it.

Mr. Christie: I should not have risen except that reference had been made to my remarks. I do not know that I should say very much, but I think I may remark with reference to this question of coincidence, that everything turns upon the exactness of the coincidence, and whether these are actually coincidences or not. I am not quite prepared to admit that these coincidences are perfect; in fact, I should say there are even coincidences of dark lines with some of the oxygen lines. I admit that it is a matter of judgment, and I should be sorry to say positively that there are such coincidences with dark lines. But there is considerable uncertainty in the matter. As Dr. Draper has explained there is a great difficulty in establishing coincidences, and you have to adjust your apparatus until you match coincidences by the known lines of iron. It seems some of these do not coincide exactly with the dark lines in the sun. I only alluded to that as being one of the difficulties we have to contend with. With regard to Mr. Ranyard's remarks as to the eye perceiving differences of brightness which the photographs do not show, I would merely wish to ask whether he has examined with his eye different parts of the spectrum, for there is a certain part in the neighborhood of the G lines which I have examined and find the photograph gives exactly the same appearance as is seen with the eye, i. e., the whole space is of equal intensity. I do not think there is much difference between the photograph and the spectrum as seen with the eye. The only object of my remarks was to clear the ground and to point out what it is we are assuming when we predicate the existence of oxygen as giving bright lines in the sun. I do not dispute it, but only point out the difficulties we must face in order that they may be fairly met.

Mr. J. Rand Capron: There is one question I should like to ask Dr. Draper, and that is, whether he had tried any experiments with the tube spectrum of oxygen?

Dr. Draper: A great many.

Mr. J. Rand Capron: The spark taken in air would probably hardly agree with the spectrum of oxygen near the sun's body. I have had occasion to photograph simultaneously the tube spectrum of hydrogen and the air spark spectrum at ordinary pressures. The tube spectrum of hydrogen showed four hydrogen lines perfect throughout, but only one of these lines was represented in the spectrum of air at ordinary pressure, so that it is possible certain oxygen lines present in the sun may be absent in the spark spectrum.

Dr. Draper: I have taken the oxygen spectrum under a great many different circumstances. I began with tubes containing oxygen and compounds of oxygen, but the difficluty is that you are limited to rather small dispersion, because you cannot get

brightness enough for a larger apparatus. Then the difficulty of having iron terminals so as to show a good coincidence is a serious one. So when I made the spark-compressor I arranged a contrivance at the back which would enable me to let in oxygen and the other gases between the terminals, and after various experiments with oxygen I find that it seems to suffer less change with altered conditions than a great many of the other elements I have experimented on. I have fairly shown that the bright lines coincide with bright spaces in the solar spectrum. The minor differences may be fairly attributed to such changes of condition as Mr. Rand Capron has referred to. With regard to Dr. Gladstone's remark, which was that probably we should not find in the chromosphere the lines of the oxygen spectrum. That is precisely what I hope will be the case, although I am going to look as hard as I can for them. I should like to see them if they are there, but I shall be better satisfied if they are not.

A cordial vote of thanks was then passed to Dr. Draper.

[The photographs of the oxygen spectrum and juxtaposed solar spectrum, were also presented to the French Academy of Sciences in Paris, at the meeting of June 23, 1879, by M. A. Cornu. M. Faye made the following remarks, which we translate from the Comptes Rendus.—EDS.]

"I cannot refrain from adding some words to the brilliant communication that the Academy has just heard. Everything leads us to believe that the constitution of the photosphere and its marvellous alimentation, are due to alternate phenomena of chemical combination and dissociation operating at divers temperatures in the mass of the Sun under the influence of ascending and descending vertical movements. Such at least is the idea I have arrived at, by the study of spots, of the problem which I think I have presented in all its fulness. Naturally, the richness in oxygen of the compounds which constitute the earth's crust, though the proportion diminishes little by little as we descend, ought to cause us to believe that this same element should play an analogous part in the Sun; but it is a remarkable thing that up to the present, spectrum analysis has not given any trace of it. On the other hand, it showed that round that star there was a vast atmosphere of hydrogen almost pure and very much rarified, of which certain portions, frequently drawn downward by the mechanical action of solar whirlwinds, gave origin, in ascending again, to the phenomena of the protuberances.

Mr. H. Draper has, however, succeeded in discovering the oxygen, not in the chromosphere, but in the photosphere, where it discloses itself by bright lines. It is obvious that if this gas

is dissociated at a depth, it is immediately taken up by multiple combinations in the region and at the temperature of the brilliant surface. I see in these facts the hope of a confirmation and above all of an extension of the views I have put forth on the constitution of the Sun; but whatever may be the fate that the progress of spectrum analysis reserves to them, I express here my admiration for the discovery of Mr. Draper, and I hope that his results, so well confirmed by the photographic proofs that our learned member M. Cornu has shown to the Academy, will not delay in being universally accepted by competent judges."

ART. XXXVII.—*On the Vapor-Densities of Peroxide of Nitrogen, Formic Acid, Acetic Acid, and Perchloride of Phosphorus;* by J. WILLARD GIBBS.

THE relation between temperature, pressure, and volume, for the vapor of each of these substances differs widely from that expressed by the usual laws for the gaseous state,—the laws known most widely by the names of Mariotte, Gay-Lussac, and Avogadro. The *density* of each vapor, in the sense in which the term is usually employed in chemical treatises, i. e., its density taken relatively to air of the same temperature and pressure,* has not a constant value, but varies nearly in the ratio of one to two. And these variations are exhibited at pressures not exceeding that of the atmosphere and at temperatures comprised between zero and 200° or 300° of the centigrade scale.

Such anomalies have been explained by the supposition that the vapor consists of a mixture of two or three different kinds of gas or vapor, which have different densities. Thus it is supposed that the vapor of peroxide of nitrogen is a gas-mixture, the components of which are represented (in the newer chemical notation) by NO_2 and N_2O_4 respectively. The densities corresponding to these formulæ are 1·589 and 3·178. The density of the mixture should have a value intermediate between these numbers, which is substantially the case with the actual vapor. The case is similar with respect to the vapor of formic acid, which we may regard as a mixture of CH_2O_2 (density 1·589) and $C_2H_4O_4$ (density 3·178), and the vapor of acetic acid, which we may regard as a mixture of $C_2H_4O_2$ (density 2·073) and $C_4H_8O_4$ (density 4·146). In the case of perchloride of phosphorus, we must suppose the vapor to consist of three parts; PCl_5 (the proper perchloride, density 7·20), PCl_3 (the protochloride, density 4·98), and Cl_2 (chlorine, density 2·22). Since the chlorine and protochloride arise from the decom-

* The language of this paper will be conformed to this usage.

position of the perchloride, there must be as many molecules of the type Cl_2 as of the type PCl_5. Now a gas-mixture containing an equal number of molecules of PCl_5 and Cl_2 will have the density $\frac{1}{2}(4{\cdot}98+2{\cdot}22)$ or 3·60. It follows that at least so far as the range of the possible values of its density is concerned, we may regard the vapor as a mixture in variable proportions of two kinds of gas having the densities 7·20 and 3·60 respectively. The observed values of the density accord with this supposition.

These hypotheses respecting the constitution of the vapors are corroborated, in the case of peroxide of nitrogen and perchloride of phosphorus, by other circumstances. The varying color of the first vapor may be accounted for by supposing that the molecules of the type N_2O_4 are colorless, while each molecule of the type NO_2 has a constant color. This supposition affords a simple relation between the density of the vapor and the depth of its color, which has been verified by experiment.*

The vapor of the perchloride of phosphorus shows with increasing temperature in an increasing degree the characteristic color of chlorine. The amount of the color appears to be such as is required by the hypothesis respecting the constitution of the vapor on the very probable supposition that the perchloride proper is colorless, but the case hardly admits of such exact numerical determinations as are possible with respect to the peroxide of nitrogen.† But since the products of dissociation are in this case dissimilar, they may be partially separated by diffusion through a neutral gas, the lighter chlorine diffusing more rapidly than the heavier protochloride. The fact of dissociation has in this way been proved by direct experiment.‡

In the case of acetic and formic acids, we have no other evidence than the variations of the densities in support of the hypothesis of the compound nature of the vapor, yet if these variations shall appear to follow the same law as those of the peroxide of nitrogen and the perchloride of phosphorus, it will be difficult to refer them to a different cause.

But however it may be with these acids, the peroxide of nitrogen and the perchloride of phosphorus evidently furnish us with the means of studying the laws of chemical equilibrium in gas-mixtures in which chemical change is possible and does in fact take place, reversibly, with varying conditions of temperature and pressure. Or, if from any considerations we can deduce a general law determining the proportions of the

* Salet, "Sur la coloration du peroxyde d'azote." Comptes Rendus, t. lxvii, p. 488.

† H. Sainte-Claire Deville, "Sur les densités de vapeur." Comptes Rendus, t. lxii, p. 1157.

‡ Wanklyn and Robinson. "On Diffusion of Vapours: a means of distinguishing between apparent and real Vapour-densities of Chemical Compounds. Proc. Roy. Soc., vol. xii, p. 507.

component gases necessary for the equilibrium of such a mixture under any given conditions, these substances afford an appropriate test for such a law.

In a former paper* by the present writer, equations were proposed to express the relation between the temperature, the pressure or the volume, and the quantities of the components in such a gas-mixture as we are considering—a *gas-mixture of convertible components* in the language of that paper. Applied to the vapor of the peroxide of nitrogen, these equations led to a formula giving the density in terms of the temperature and pressure, which was shown to agree very closely with the experiments of Deville and Troost, and much less closely, but apparently within the limits of possible error, with the experiments of Playfair and Wanklyn. Since the publication of that paper, new determinations of the density have been published in different quarters, which render it possible to compare the equation with the results of experiment throughout a wider range of temperature and pressure. In the present paper, all experimental determinations of the density of this vapor which have come to the knowledge of the writer are cited, and compared with the values demanded by the formula, and a similar comparison of theory and experiment is made with respect to each of the other substances which have been mentioned.

The considerations from which these formulæ were deduced may be briefly stated as follows. It will be observed that they are based rather upon an extension of generally acknowledged principles to a new class of cases than upon the introduction of any new principle.

The energy of a gas-mixture may be represented by an expression of the form

$$m_1 (c_1 t + \mathrm{E}_1) + m_2 (c_2 t + \mathrm{E}_2) + \text{etc.},$$

with as many terms as there are different kinds of gas in the mixture, m_1, m_2, etc., denoting the quantities (by weight) of the several component gases, c_1, c_2, etc., their several specific heats at constant volume, E_1, E_2, etc., other constants, and t the absolute temperature. In like manner the entropy of the gas-mixture is expressed by

$$m_1 \left(\mathrm{H}_1 + c_1 \log_N t - a_1 \log_N \frac{m_1}{v} \right) + m_2 \left(\mathrm{H}_2 + c_2 \log_N t - a_2 \log_N \frac{m_2}{v} \right) + \text{etc.},$$

* "On the Equilibrium of Heterogeneous Substances." Transactions of the Connecticut Academy, vol. iii, p. 108. The equations referred to are (313), (317), (319) and (320), on pages 233 and 234. The applicability of these equations to such cases as we are now considering is discussed under the heading "Gas-mixtures with Convertible Components," page 234.

where v denotes the volume, and H_1, a_1, H_2, a_2, etc., denote constants relating to the component gases, a_1, a_2, etc., being inversely proportional to their several densities. The logarithms are Naperian. These expressions for energy and entropy will undoubtedly apply to mixtures of different gases, whatever their chemical relations may be, (with such limitations and with such a degree of approximation as belong to other laws of the gaseous state), when no chemical action can take place under the conditions considered. If we assume that they will apply to such cases as we are now considering, although chemical action is possible, and suppose the equilibrium of the mixture with respect to chemical change to be determined by the condition that its entropy has the greatest value consistent with its energy and its volume, we may easily obtain an equation between m_1, m_2, etc., t and v.*

The condition that the energy does not vary, gives

$$(m_1 c_1 + m_2 c_2 + \text{etc.})\, dt + (c_1 t + E_1)\, dm_1 + (c_2 t + E_2)\, dm_2 + \text{etc.} = 0. \qquad (1)$$

The condition that the entropy is a maximum implies that its variation vanishes, when the energy and volume are constant: this gives

$$\frac{m_1 c_1 + m_2 c_2 + \text{etc.}}{t}\, dt + \left(H_1 - a_1 + c_1 \log_N t - a_1 \log_N \frac{m_1}{v}\right) dm_1 + \left(H_2 - a_2 + c_2 \log_N t - a_2 \log_N \frac{m_2}{v}\right) dm_2 + \text{etc.} = 0. \qquad (2)$$

Eliminating dt, we have

$$\left(H_1 - a_1 - c_1 - \frac{E_1}{t} + c_1 \log_N t - a_1 \log_N \frac{m_1}{v}\right) dm_1 + \left(H_2 - a_2 - c_2 - \frac{E_2}{t} + c_2 \log_N t - a_2 \log_N \frac{m_2}{v}\right) dm_2 + \text{etc.} = 0. \qquad (3)$$

If the case is like that of the peroxide of nitrogen, this equation will have two terms, of which the second may refer to the denser component of the gas-mixture. We shall then have $a_1 = 2a_2$, and $dm_1 = -dm_2$, and the equation will reduce to the form

$$\log \frac{m_2 v}{m_1^2} = -A - B \log t + \frac{C}{t}, \qquad (4)$$

where common logarithms have been substituted for Naperian, and A, B and C are constants. If, in place of the quantities of the components, we introduce the partial pressures, p_1, p_2, due to these components, and measured in millimeters of mercury, by means of the relations

* For certain *a priori* considerations which give a degree of probability to these assumptions, the reader is referred to the paper already cited.

$$m_1 = \frac{p_1 v}{\alpha_1 t}, \qquad m_2 = \frac{p_2 v}{\frac{1}{2}\alpha_1 t},$$

where α_1 denotes a constant, we have

$$\log \frac{p_2}{p_1^2} = -(A + \log 2\alpha_1) - (1 + B)\log t + \frac{C}{t}$$
$$= -A' - B' \log t + \frac{C}{t}, \qquad (5)$$

where A′ and B′ are new constants. Now if we denote by p the total pressure of the gas-mixture (in millimeters of mercury), by D its density (relative to air of the same temperature and pressure), and by D_1 the theoretical density of the rarer component, we shall have

$$p : p + p_2 :: D_1 : D.$$

This appears from the consideration that $p + p_2$ represents what the pressure would become, if without change of temperature or volume all the matter in the gas-mixture could take the form of the rarer component. Hence,

$$p_2 = p\,\frac{D - D_1}{D_1},$$

$$p_1 = p - p_2 = p\,\frac{2D_1 - D}{D_1},$$

and

$$\frac{p_2}{p_1^2} = \frac{D_1 (D - D_1)}{p (2D_1 - D)^2}.$$

By substitution in (5) we obtain

$$\log \frac{D_1 (D - D_1)}{(2D_1 - D)^2} = -A' - B' \log t + \frac{C}{t} + \log p_1. \qquad (6)$$

By this formula, when the values of the constants are determined, we may calculate the density of the gas-mixture from its temperature and pressure. The value of D_1 may be obtained from the molecular formula of the rarer component. If we compare equations (3), (4) and (5), we see that

$$B' = B + 1, \qquad B = \frac{c_1 - c_2}{a_2}.$$

Now $c_1 - c_2$ is the difference of the specific heats at constant volume of NO_2 and N_2O_4. The general rule that the specific heat of a gas at constant volume and per unit of weight is independent of its *condensation*, would make $c_1 = c_2$, $B = 0$, and $B' = 1$. It may easily be shown, with respect to any of the substances considered in this paper,* that unless the numerical value of B′ greatly exceeds unity, the term B′ log t may be neglected without serious error, if its omission is compensated

* For the case of peroxide of nitrogen, see pp. 243, 244 in the paper cited above.

in the values given to A′ and C. We may therefore cancel this term, and then determine the remaining constants by comparison of the formula with the results of experiment.

In the case of a mixture of Cl_2, PCl_3 and PCl_5, equation (3) will have three terms distinguished by different suffixes. To fix our ideas, we may make these suffixes $_2$, $_3$ and $_5$, referring to Cl_2, PCl_3 and PCl_5 respectively. Since the constants a_2, a_3 and a_5 are inversely proportional to the densities of these gases,

$$a_2\,dm_2 = a_3\,dm_3 = -a_5\,dm_5,$$

and we may substitute $\frac{1}{a_2}, \frac{1}{a_3}, \frac{-1}{a_5}$ for dm_2, dm_3 and dm_5 in equation (3), which is thus reduced to the form

$$\log \frac{m_5 v}{m_2 m_3} = -\mathrm{A} - \mathrm{B}\log t + \frac{\mathrm{C}}{t}. \tag{7}$$

If we eliminate m_2, m_3, m_5 by means of the partial pressures, p_2, p_3, p_5, we obtain

$$\log \frac{p_5}{4 p_2 p_3} = -\mathrm{A}' - \mathrm{B}'\log t + \frac{\mathrm{C}}{t}, \tag{8}$$

when A′, B′, like A, B and C, are constants. If the chlorine and the protochloride are in such proportions as arise from the decomposition of the perchloride, $p_2 = p_3$ and $4p_2p_3 = (p_2+p_3)^2$. In this case, therefore, we have

$$\log \frac{p_5}{(p_2+p_3)^2} = -\mathrm{A}' - \mathrm{B}'\log t + \frac{\mathrm{C}}{t}. \tag{9}$$

It will be seen that this equation is of the same form as equation (5), when p_5 in (9) is regarded as corresponding to p_2 in (5), and p_2+p_3 in (9), which represents the pressure due to the products of decomposition, is regarded as corresponding to p_1 in (5), which has the same signification. It follows that equation (5), as well as (6), which is derived from it, may be regarded as applying to the vapor of perchloride of phosphorus, when the values of the constants are properly determined. This result might have been anticipated, but the longer course which we have taken has given us the more general equations, (7) and (8), which will apply to cases in which there is an excess of chlorine or of the protochloride.

If the gas-mixture considered, in addition to the components capable of chemical action, contains a neutral gas, the expressions for the energy and entropy of the gas-mixture should properly each contain a term relating to this neutral gas. This would make it necessary to add $c_n m_n$ to the coefficient of dt in (1), and $\frac{c_n m_n}{t}$ to the coefficient of dt in (2), the suffix $_n$ being

used to mark the quantities relating to the neutral gas. But these quantities would disappear with the elimination of dt, and equation (3) and all the subsequent equations would require no modification, if only p and D are estimated (in accordance with usage) with exclusion of the pressure and weight due to the neutral gas. This result, which may be extended to any number of neutral gases, is simply an expression of Dalton's Law.

We now proceed to the comparison of the formulæ, especially of equation (6), with the results of experiment.

TABLE I. — PEROXIDE OF NITROGEN.

Experiments at Atmospheric Pressure.

MITSCHERLICH, — R. MÜLLER — DEVILLE and TROOST.

Temperature.	Pressure.	Density calc. by eq. (10).	Density observed.				Excess of observed density.			
			M—h. M—r.	Deville & Troost. I.	II.	III.	M—h. M—r.	Deville & Troost. I.	II.	III.
183·2	(760)	1·592				1·57				—·022
154·0	(760)	1·597				1·58				—·017
151·8	(760)	1·598		1·50				—·10		
135·0	(760)	1·607				1·60				—·007
121·8	(760)	1·622		1·64				+·02		
121·5	(760)	1·622				1·62				—·002
111·3	(760)	1·641				1·65				+·009
100·25	760	1·677	1·72				+·04			
100·1	(760)	1·676				1·68				+·004
100·0	(760)	1·677		1·71				+·03		
90·0	(760)	1·728				1·72				—·008
84·4	(760)	1·768		1·83				+·06		
80·6	(760)	1·801				1·80				—·001
79	748	1·814	1·84				+·03			
77·4	(760)	1·833			1·85				+·02	
70·0	(760)	1·920				1·92				·000
70	754·5	1·919	1·95				+·03			
68 8	(760)	1·937		1·99				+·05		
66·0	(760)	1·976			2·03				+·05	
60·2	(760)	2·067				2·08				+·013
55·0	(760)	2·157			2·20				+·04	
52	757	2·211	2·26				+·05			
49·7	(760)	2·255		2·34				+·09		
49·6	(760)	2·256				2·27				+·014
45·1	(760)	2·342			2·40				+·06	
39·8	(760)	2·443				2·46				+·017
35·4	(760)	2·524				2·53				+·006
35·2	(760)	2·528		2·66				+·13		
34·6	(760)	2·539			2·62				+·08	
32	748	2·582	2·65				+·07			
28·7	(760)	2·642		2·80				+·16		
28	751	2·652	2·70				+·05			
27·6	(760)	2·661			2·70				+·04	
26·7	(760)	2·676				2·65				—·026

Peroxide of nitrogen.—If we take the constants of the equation for this substance from the paper already cited,* we have

$$\log \frac{15{\cdot}89\,(D - 1{\cdot}589)}{(3{\cdot}178 - D)^2} = \frac{3118{\cdot}6}{t_C + 273} + \log p - 12{\cdot}451, \quad (10)$$

t_C denoting the temperature on the centigrade scale. The numbers 3·178 and 1·589 represent the theoretical densities of N_2O_4 and NO_2 respectively. The two other constants were determined by the experiments of Deville and Troost.

The results of these and other experiments at atmospheric pressure, all made by Dumas' method, are exhibited in Table I. The first three columns give the temperature (centigrade), the pressure (in millimeters of mercury),† and the density calculated from the temperature and pressure by equation (10). The subsequent columns give the densities observed by different authorities, and the excess of the observed over the calculated densities. In the first column of observed densities, we have one observation by Mitscherlich‡ (at 100·25°) and five by R. Müller.§ The three remaining columns contain each the results of a series of experiments by Deville and Troost.‖ In each series the experiments were made with increasing temperatures, and with the same vessel, without refilling. It should be observed that the results of the three series are not regarded by their distinguished authors as of equal weight. It is expressly stated that the numbers in the two earlier series, and especially in the first, may be less exact. The last series agrees very closely with the formula. It was from this that the constants of the formula were determined. The experiments of series I and II, and those of Mitscherlich and Müller, give somewhat larger values, with a single exception, as is best seen in the columns which give the excess of the observed density. The differences between the different columns are far too regular to be attributed to the accidental errors of the individual observations, except in the case of the experiment at 151·8°,

* See equation (336) on page 339, *loc. cit.*,—also the following equations in which the density is given in terms of the temperature and pressure. In comparing these equations, it must be observed that in (336) the pressures are measured in atmospheres, but in this paper in millimeters of mercury.

† 760^{mm} has been assumed as the pressure of the atmosphere in all cases in which the precise pressure is not recorded in the published account of the experiments. The figures inserted in the columns of pressures are in such cases enclosed in parentheses. The same course has been followed in the subsequent tables. With respect to the principal series of observations by Deville and Troost (series III), it is stated that the barometer varied between 747 and 764 millimeters. A difference of 13 millimeters in the pressure would in no case cause a difference of ·005 in the calculated densities. In this series, therefore, the errors due to this circumstance are not very serious.

‡ Pogg. Ann., vol. xxix (1833), p. 220.

§ Lieb. Ann., vol. cxxii (1862), p. 15.

‖ Comptes Rendus, vol. lxiv (1867), p. 237.

where some accident has evidently occurred either in the experiment itself or in the reduction of the result. Setting this observation aside, we must look for some constant cause for the other discrepancies between the different series.

We can hardly attribute these discrepancies to difference in the material employed, or to air or other foreign substance imperfectly expelled from the flask. For impurities which increase the density would make the divergence between the different series greatest when the densities are the least, whereas the divergences seem to vanish as the density approaches the limiting value. (A similar objection would apply to the supposition of any error in the determination of the weight of the flask when filled with air alone.) But if we should attribute the divergences to an impurity which diminishes the density (as air), we should be driven to the conclusion that the first series of Deville and Troost gives the most correct results, and that all the best attested numbers at temperatures below 90° are considerably in the wrong. It does not seem possible to account for these discrepancies by any causes which would apply to cases of normal or constant density. They are illustrations of the general fact that when the density varies rapidly with the temperature, determinations of density for the same temperature and pressure by different observers, or different determinations by the same observer, exhibit discordances which are entirely of a different order of magnitude from those which occur with substances of normal or constant densities, or which occur with the same substance at temperatures at which the density approaches a constant value. In some cases such results may be accounted for by carelessness on the part of the observers, not controlled by a comparison of the result with a value already known. But such an explanation is inadequate to explain the general fact, and evidently inadmissible in the present case.

It is probable that these discrepancies are in part attributable to a circumstance which has been noticed by M. Wurtz, in his account of his experiments upon the vapor-density of bromhydrate of amylene, in the following words: "Le temps pendant lequel la vapeur est maintenue à la température où l'on détermine la densité n'est pas sans influence sur les nombres obtenus. C'est ce qui résult des deux expériences faites à 225 degrés avec des produits identiques. Dans la première, la vapeur a été portée rapidement à 225 degrés. Dans la seconde elle a été maintenue pendant dix minutes à cette température. On voit que les nombres trouvés pour les densités ont été fort différents. [The numbers were 4·69 and 3·68 respectively.] Ce résultat ne doit point surprendre si l'on considère que le phénomène de décomposition de la vapeur doit absorber de la chaleur, et que les quantités de chaleur nécessaires pour pro-

duire et la dilatation et la décomposition ne sauraient être fournies instantanément."*

It is not difficult to form an estimate of the quantities of heat which come into play in such cases. With respect to peroxide of nitrogen, it was estimated in the paper already cited that the heat absorbed in the conversion of a unit of N_2O_4 into NO_2 under constant pressure is represented by $7181\ a_2$. (The heat is supposed to be measured in units of mechanical work). Now the external work done by the conversion of a unit of N_2O_4 into NO_2 under constant pressure is $a_2 t$. Therefore, the ratio of the heat absorbed to the external work done by the conversion of N_2O_4 into NO_2 is $7181 \div t$, or 23 at the temperature of 40° centigrade. Let us next consider how much more rapidly this vapor expands with increase of temperature at constant pressure than air. From the necessary relation

$$v = \frac{k\,m\,t}{p\,\mathrm{D}},$$

where m denotes the weight of the vapor, and k a constant, we obtain

$$\left(\frac{dv}{dt}\right)_p = \frac{v}{t} - \frac{v}{\mathrm{D}}\left(\frac{d\mathrm{D}}{dt}\right)_p,$$

where the suffix $_p$ indicates that the differential coefficients are for constant pressure. The last term of this expression evidently denotes the part of the expansion which is due to the conversion of N_2O_4 into NO_2, and the preceding term the expansion which would take place if there were no such conversion, and which is identical with the expansion of the same volume of air under the same circumstances. The ratio of the two terms is $-\frac{t}{\mathrm{D}}\left(\frac{d\mathrm{D}}{dt}\right)_p$, the numerical value of which for the temperature of 40° is 2·42, as may be found by differentiating equation (10), or, with less precision, from the numbers in the third column of Table I. Let us now suppose that equal volumes of peroxide of nitrogen and of air at the temperature of 40° and the pressure of one atmosphere receive equal infinitesimal increments of temperature under constant pressure. The heat absorbed by the peroxide of nitrogen on account of the conversion of N_2O_4 into NO_2 is 23 times the external work due to the same cause, and this work is 2·42 times the external work done by the expansion of the air. But the heat absorbed by the air in expanding under constant pressure is well known to be 3·5 times the work done. Therefore the heat absorbed on account of the conversion of N_2O_4 into NO_2 is $(23 \times 2{\cdot}42 \div 3{\cdot}5 =) 15{\cdot}9$ times the heat absorbed by the air. To obtain the

* Comptes Rendus, t. lx, p. 730.

whole heat absorbed by the vapor we must add that which would be required if no conversion took place. At 40° the vapor of peroxide of nitrogen contains about 54 molecules of N_2O_4 to 46 of NO_2, as may easily be calculated from its density. The specific heat for constant pressure of a mixture in such proportions of gases of such molecular formulæ, if no chemical action could take place, would be about twice that of the same volume of air. Adding this to the heat absorbed by the chemical action we obtain the final result,—that at 40° and the pressure of the atmosphere the specific heat of peroxide of nitrogen at constant pressure is about eighteen times that of the same volume of air.*

But the greater amount of heat which is required to bring the vapor to the desired temperature is only one factor in the increased liability to error in cases of this kind. The expansion of peroxide of nitrogen for increase of temperature under constant pressure at 40° is 3·42 times that of air. If then, in a determination of density, the vapor fails to reach the temperature of the bath, the error due to the difference of the temperature of the vapor and the bath, will be 3·42 times as great as would be caused by the same difference of temperatures in the case of any vapor or gas having a constant density. When we consider that we are liable not only to the same, but to a much greater difference of temperatures in a case like that of peroxide of nitrogen, when the exposure to the heat is of the same duration, it is evident that the common test of the exactness of a process for the determination of vapor-densities, by applying it to a case in which the density is nearly constant, is entirely insufficient.

That the experiments of the III[d] series of Deville and Troost give numbers so regular and so much lower than the other experiments is probably to be attributed in part to the length of time of exposure to the heat of the experiment, which was half an hour in this series,—for the other series, the time is not given.

Another point should be considered in this connection. During the heating of the vapor in the bath, it is not immaterial whether the flask is open or closed. This will appear, if we compare the values of $\left(\frac{dD}{dt}\right)_p$ and $\left(\frac{dD}{dt}\right)_v$, the differential coefficients of the density with respect to the temperature on the suppositions, respectively, of constant pressure, and of constant volume. For 40°, we have

$$\left(\frac{dD}{dt}\right)_p = \cdot 0189, \qquad \left(\frac{dD}{dt}\right)_v = \cdot 0163,$$

* Similar calculations from less precise data for the bromhydrate of amylene at 225° seem to indicate a specific heat as much as forty times as great as that of the same volume of air.

the first number being obtained immediately from equation (10) by differentiation, and the second by differentiation after substitution of $\frac{kmt}{vD}$ for p. The ratio of these numbers evidently gives the proportion in which the chemical change takes place under the two suppositions. This shows that only about six-sevenths of the heat required for the chemical change can be supplied before opening the flask, and the remainder of this heat as well as that required for expansion must be supplied after the opening. The errors due to this source may evidently be diminished by diminishing the intervals of temperature between the successive experiments in a series of this kind, and also by diminishing the opening made in the flask, which increases the time for which the flask may be left open without danger of the entrance of air. In the III[d] series of experiments by Deville and Troost, the intervals of temperature did not exceed ten degrees (except after the density had nearly reached its limiting value), and the neck of the flask was drawn out into a very fine tube.

In Table II, which relates to experiments on the same substance at pressures less than that of the atmosphere, the principal series is that of Naumann,* which commences a few degrees below the lowest temperatures of Deville and Troost, and extends to $-6°$ centigrade, the pressures varying from 301 to 84 millimeters. These experiments were made by the method of Gay-Lussac. The numbers in the column of observed densities have been re-calculated from the more immediate results of the experiments, and are not in all cases identical with those given in Professor Naumann's paper. Every case of difference is marked with brackets. Instead of the numbers [2·66], [2·62], [2·85], [2·94], Naumann's paper has 2·57, 2·65, 2·84, 3·01, respectively. In some cases the temperatures and pressures of two experiments are so nearly the same that it would be allowable to average the results, at least in the column of excess of observed density. In such cases the numbers in this column have been united by a brace. The greatest difference between the observed and calculated densities is ·16, which occurs at the least pressure, 84 millimeters. In this experiment the weight of the substance employed is also less than in any other experiment. Under such circumstances, the liability to error is of course greatly increased. The average difference between the observed and calculated densities is ·063. Since these differences are almost uniformly positive and increase as the temperature diminishes, it is evident that they might be considerably diminished by slight changes in the constants of equation (10), without seriously impairing the agreement of that equa-

* Berichte der Deutschen Chemischen Gesellschaft, Jahrgang xi (1878), S. 2045.

tion with the experiments of Deville and Troost. But it has not seemed necessary to re-calculate the formula, which, in its present form, will at least illustrate the degree of accuracy with which densities at low pressures and at temperatures below the boiling point of the liquid may be derived from experiments at atmospheric pressure above the boiling point. Moreover, the excess of observed density may be due in part to a circumstance mentioned by Professor Naumann, that the chemical action between the vapor and the mercury diminished the volume of the vapor, and thus increased the numbers obtained for the density.

TABLE II.—PEROXIDE OF NITROGEN.

Experiments at less than Atmospheric Pressure.

PLAYFAIR and WANKLYN,—TROOST,—NAUMANN.

Temperature.	Pressure.	Density calculated by eq. (10).	Density observed.			Excess of obs. density.		
			P. & W.	T.	N.	P. & W.	T.	N.
97·5	(301)	1·631	1·783			+·152		
27	35	1·90		1·6			−·30	
27	16	1·77		1·59			−·18	
24·5	(323)	2·524	2·52			−·004		
22·5	136·5	2·34			2·35			+·01
22·5	101	2·26			2·28			+·02
21·5	161	2·41			2·38			−·03
20·8	153·5	2·41			2·46			+·05
20	301	2·59			2·70			+·11
18·5	136	2·43			2·45			+·02
18	279	2·61			2·71			+·10
17·5	172	2·51			2·52			+·01
16·8	172	2·53			2·55			+·02
16·5	224	2·59			[2·66]			+·07
16	228·5	2·61			[2·62]			+·01
14·5	175	2·58			2·63			+·05
11·3	(159)	2·620	2·645			+·025		
11	190	2·66			2·76			+·10
10·5	163	2·64			2·73			+·09
4·2	(129)	2·710	2·588			−·122		
4	172·5	2·77			2·85			+·08
2·5	145	2·76			[2·85]			+·09
1	138	2·78			2·84			+·06
−1	153	2·83			2·87			+·04
−3	84	2·76			2·92			+·16
−5	123	2·85			2·98			+·13
−6	125·5	2·87			[2·94]			+·07

The same table includes two experiments of Troost,* by Dumas' method, but at the very low pressures of 35^{mm} and 16^{mm}. In such experiments we cannot expect a close agreement with the formula, for the same error in the determination of the weight of the vapor, which would make a difference of

* Comptes Rendus, t. lxxxvi (1878), p. 1395.

·01 in the density in experiments at atmospheric pressure, would make a difference of ·21 or ·47 in the circumstances of these experiments. In fact, the numbers obtained differ considerably from those demanded by the formula.

There remain four experiments by Playfair and Wanklyn* in which Dumas' method was varied by diluting the vapor with nitrogen. The numbers in the column of pressures represent the total pressure diminished by the pressure which the nitrogen alone would have exerted. They are not quite accurate, since the data given to the memoir cited only enable us to determine the ratios of the total and the partial pressures. The numbers here given are obtained by setting the total pressure, which was that of the atmosphere at the time of the experiment, equal to 760^{mm}. The effect of this inaccuracy upon the calculated densities would be small. Two of these observations agree closely with the formula; and two show considerable divergence, but in opposite directions, and these are the two in which the quantities of peroxide of nitrogen were the smallest. The differences appear to be attributable rather to the difficulty of a precise determination of the quantities of nitrogen and of vapor, than to any effect of the one upon the other.

Special interest attaches to experiments at the same or nearly the same temperature but different pressures. For with experiments at the same temperature, the constants of the formula which are determined by observation are reduced to one, so that the verification of the formula by experiment cannot possibly be regarded as a case of interpolation. It is not necessary that the temperatures should be exactly the same, for it will be conceded that the formula represents the actual function well enough to answer for adjusting slight differences of temperature; but it is necessary that the range of pressures should be considerable in order that the differences of density should be large in proportion to the probable errors of observation. But the pressures must not be so low that accurate determinations become impossible.

In the experiments of Naumann we see some fair correspondences with the formula in respect to the influence of pressure, especially in the first four experiments of the list, where, if we average the results of the third and fourth experiments, as is evidently allowable, the observed values follow very closely the fluctuations of the calculated, extending from 2·26 to 2·41. In other cases the agreement is less satisfactory. The circumstance that the experiments at the two highest pressures (301 and 279^{mm}), give results exceeding the calculated values considerably more than any other experiments at adjacent temperatures may seem to indicate that the densities

* Trans. Roy. Soc. Edinb., vol. xxii (1861), p. 463.

increase with the pressures more rapidly than the formula allows; but the differences are not too large to be ascribed to errors of observation, and the experiment at the lowest pressure (84^{mm}) also shows a large excess of observed density.

A much more critical test may be found in the comparison of Naumann's experiments with those of Deville and Troost, notwithstanding the interval of about 4° of temperature. The formula requires that a diminution of pressure from 760 to 101 millimeters shall reduce the density from 2·676 at 26·7° to 2·26 at 22·5, notwithstanding the effect of the change of temperature. Experiment gives a reduction of density from 2·65 to 2·28, which is about one-ninth less. This is, it will be observed, a deviation from the formula in the opposite direction from that which the experiments of Naumann alone, or a comparison of the experiments of Troost with those of Deville and Troost, seemed to indicate. The experiment here compared with Naumann's belongs to the III^d series of Deville and Troost. If instead of this experiment we should take an average of the experiments at lowest temperature in the II^d and III^d series, the agreement with the formula with respect to the effect of change of pressure would be almost perfect.

Formic acid.—In Table III, the determinations of Bineau are compared with the densities calculated by the formula

$$\log \frac{1{\cdot}589\,(D - 1{\cdot}589)}{(3{\cdot}178 - D)^2} = \frac{3800}{t_c + 273} + \log p - 12{\cdot}641. \qquad (11)$$

The observed densities are taken from the eighteenth volume of the third series of the Annales de Chimie et de Physique (1846), except in three cases, distinguished by parentheses, which are earlier determinations published in the nineteenth volume of the Comptes Rendus (1844). It may be added that the pressure (687) for the experiment at 108° is taken from Erdmann's Journal für praktische Chemie (vol. xl, p. 44), the impression being imperfect in the Annales, in the copies to which the writer has been able to refer, where the figures look much like 637. (The pressure 637 would make the calculated density 2·28.)

In the column which gives the excess of observed densities, the effect of nearness to the state of saturation is often very marked. Such cases are distinguished by an asterisk. The temperature of 99·5° is below the boiling point of formic acid, and the higher pressures employed at this temperature cannot be far from the pressure of saturated vapor. With respect to lower temperatures, we have the statement of Bineau that the pressure of saturated vapor is about 19^{mm} at 13°, $20{\cdot}5^{mm}$ at 15°, $33{\cdot}5^{mm}$ at 22°, and $53{\cdot}5^{mm}$ at 32°. By interpolation between the *logarithms* of these pressures, (in a single case, by *extrapolation*), we obtain the following result.

Temperature.....	10·5	12·5	16	18·5	22
Pressure of sat. vapor...	16·6	18·5	22	26·2	33·5
Pressure of experiment..	14·69	15·20	15·97	23·53	25·17

Table III. — Formic Acid.

Experiments of Bineau.

Temperature.	Pressure.	Density calculated by eq. (11).	Density observed.	Excess of observed density.
216·0	690	1·60	1·61	+·01
184·0	750	1·64	1·68	+·04
125·5	687	2·03	2·05	+·02
125·5	645	2·02	2·03	+·01
124·5	670	2·04	2·06	+·02
124·5	640	2·03	2·04	+·01
118·0	655	2·13	(2·14)	(+·01)
118·0	650	2·13	2·13	·00
117·5	688	2·15	2·13	−·02
115·5	649	2·17	2·20	+·03
115·5	640	2·16	2·16	·00
115	655	2·18	(2·13)	(−·05)
111·5	690	2·25	2·22	−·03
111·5	690	2·25	2·25	·00
111	608	2·22	(2·13)	(−·09)
108	[687]	2·30	2·31	+·01
105·0	691	2·35	2·35	·00
105·0	650	2·34	2·33	−·01
105·0	630	2·33	2·32	−·01
101·0	693	2·42	2·44	+·02
101·0	650	2·40	2·41	+·01
99·5	690	2·44	2·52	+·08*
99·5	684	2·44	2·49	+·05
99·5	676	2·44	2·46	+·02
99·5	662	2·43	2·44	+·01
99·5	641	2·42	2·42	·00
99·5	619	2·41	2·41	·00
99·5	602	2·41	2·40	−·01
99·5	557	2·39	2·34	−·05
34·5	28·94	2·82	2·77	−·05
31·5	3·04	2·40	2·60	+·20
30·5	8·83	2·67	2·69	+·02
30·0	18·28	2·81	2·76	−·05
29·0	27·40	2·88	2·83	−·05
24·5	17·39	2·88	2·86	−·02
22·0	25·17	2·95	3·05	+·10*
20·0	16·67	2·93	2·94	+·01
20·0	7·99	2·84	2·85	+·01
20·0	2·72	2·64	2·80	+·16
18·5	23·53	2·98	3·23	+·25*
16·0	15·97	2·97	3·13	+·16*
15·5	2·61	2·72	2·86	+·14
15·0	7·60	2·90	2·93	+·03
12·5	15·20	3·00	3·14	+·14*
11·0	7·26	2·95	3·02	+·07
10·5	14·69	3·01	3·23	+·22*

Whether the large excess of observed density in these cases represents a property of the vapor, or an incipient condensation

on the walls of the vessel which contains it, as has been supposed by eminent physicists in similar cases, we need not here discuss.

If we reject these cases of nearly saturated vapor, as well as the three earlier determinations, there remain 25 experiments at pressures somewhat less than one atmosphere in which the maximum difference between the observed and calculated densities is ·05, and the average difference ·016; nine experiments at pressures ranging from 29^{mm} to 7^{mm}, in which the maximum difference is ·07 and the average ·035; and three experiments at pressures of about 3^{mm}, in which the average difference is ·17. The extraordinary precision of the determinations at low pressures is doubtless due to the large scale on which the experiments were conducted. All the experiments at temperatures below 99°, were made with a globe of the capacity of $5\frac{1}{2}$ liters with a stem of suitable length to hold the barometric column.

The agreement is certainly as good as could be desired, and shows the accuracy of which the method of observation is capable. But in no part of the thermometric scale do we find so great a range of pressures as might be desired, without using pressures too low for accurate results, or observations which are to be rejected for other reasons.

[To be continued.]

ART. XXXVIII.—*The Fault at Rondout;* by T. NELSON DALE, Jr.

MATHER gives a section,* from between Glasco and Great Falls of Esopus Creek, in which the Hudson River slates and the Lower Helderberg limestones are unconformably superposed, and the latter present the form of a ruptured and eroded anticlinal. He gives also, Plate 8, fig. 7, a rough and unsatisfactory section of "High Rock," situated opposite Rhinebeck, between the junction of the Wallkill and the Hudson. In this section the Lower Helderberg limestones are represented in an almost erect position, resting unconformably upon Hudson River grits.

Mr. J. G. Lindsley, the agent of the Cement Co. at Rondout, read an interesting paper† before the Poughkeepsie Society of Natural Science on Feb. 26, 1879, which contains the results of local studies for which he has had excellent opportunities.

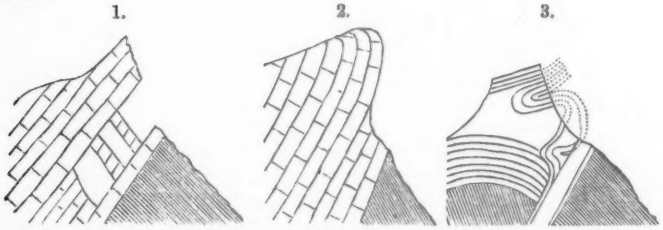
In the summer of 1878, I made several hasty visits to the highly instructive locality at Rondout, and obtained the following data, some of which, as well as the inferences from them, are not given in Mr. Lindsley's paper. The southeast base of

*Nat. Hist. of N. Y., Part IV, Geology, by W. W. Mather. Pl. 7, fig. 9, 1843.

†"A Study of the Rocks," by James G. Lindsley, in the forthcoming Part I, vol. ii, of Proceedings of the Poughkeepsie Society of Natural Science.

the hill consists of grit dipping east-northeast at an angle of about 45°. These grits form part of the series of clay-slates and grits of the Hudson River group.* Unconformably resting upon these grits is a series of limestones, dipping at about the same angle, but toward the northwest. The grits are non-fossiliferous at this point. The limestones are abundantly fossiliferous. I am indebted to Mr. Lindsley for the following measurements of the latter.

Beginning above: No. 9, 20 feet or more, Upper Pentamerus limestone. No. 8, 20 feet, Encrinal limestone. No. 7, 15–20 feet, Delthyris limestone. No. 6, 20 feet Lower Pentamerus limestone with chert, *Pentamerus galeatus*, Crinoids. No. 5, 6 feet, Ribbon limestone with *Stromatopora concentrica*. No. 4, 15 feet, Tentaculite limestone with *Tentaculites irregularis*, *Leperditia alta*. No. 3, 4 feet, limestone with prismatic mud cracks. No. 2, 30 feet, Water-lime with *Leperditia alta*. No. 1, 6 feet, Encrinal limestone. Whatever may be thought as to the presence in No. 1 and 2 of Niagara beds, we certainly have to do with Lower Helderberg in part of No. 2 and in No. 3 and 4. The relative position of the rocks is shown in the accompanying sections. For Section No. 3, I am indebted to Mr. Lindsley. The excavations represented in figure 1 were made in quarrying, the rock being a hydraulic limestone or "cement rock."



Figs. 1 to 3. Lower Helderberg beds resting unconformably on the Hudson River group (fine-lined portion in the figures); in figs. 1 and 2, dip of L. H. northwest, of H. R. east-northeast.

It has been supposed, I believe by Sir William Logan, that the great fault which begins near Quebec, crosses the Hudson near Rhinebeck, and there separates the Quebec from the true Hudson River beds.† It would appear that this is erroneous and that the facts of the case at these localities are simply these. At the close of the Hudson River Period, the Lower Silurian beds were powerfully folded and metamorphosed. A period of uplift and erosion followed; then a depression and the deposition

*See this Journal, III, vol. xvii, 1879, page 57, "On the Age of the Clay-slates and Grits of Poughkeepsie," by T. Nelson Dale, Jr.

† Dana, Manual of Geology, page 184, 2d edition, 1876.

of the Upper Silurian series. At some later time, probably at the period of the Appalachian revolution, the whole series, i. e. the Hudson River and Lower Helderberg formations, were uplifted, folded, fractured, faulted and tilted.

The complicated stratification of Section 3, may perhaps be accounted for as follows. First, unconformable deposition of Lower Helderberg upon Hudson River beds; then the formation of an anticlinal, and at the same time, closely adjoining it, a treble fold. The folds by the continued pressure were broken off from the anticlinal and thrown into an erect position. The same force has also compressed the lowest fold between the edges of the anticlinal on one side, and the surface of other Lower Helderberg strata on the other. The uppermost layers of this fold were by this action ruptured. This is not shown in the cut. As the upper portion of the base of the hill has been quarried away, the segments of three folds are now in view, one above another. The thickness of the contorted strata has been much reduced by pressure.

ART. XXXIX.—*On the Chemical Composition of Amblygonite;* by SAMUEL L. PENFIELD.

THE new mineral species triploidite described by Messrs. Brush and Dana* is shown by them to be isomorphous with wagnerite and closely related in composition to triplite. These three minerals have respectively the formulas $(Mn,Fe)_3P_2O_8+(Mn,Fe)(OH)_2$, $Mg_3P_2O_8+MgF_2$ and $(Fe,Mn)_3P_2O_8+(Fe,Mn)F_2$. From a comparison of these formulas it is argued (l. c., p. 45) that the relation between the minerals requires the assumption that the hydroxyl in triploidite must play the same part as the fluorine in the other two.

In this paper I wish to show that in amblygonite the hydroxyl group is also isomorphous with fluorine, and that in chemical composition the original amblygonite does not differ from the American and Montebras varieties which have been called hebronite. I shall also show that the results of my analyses require the adoption of a new formula for the mineral, more simple than that previously accepted. For analysis I have selected specimens from the three localities in Maine, from Branchville, Connecticut, where the mineral has been lately discovered by Messrs. Brush and Dana, also two varieties from Montebras and one from Penig, Saxony, from a specimen in the Yale College collection.

The analyses are arranged so as to form a series, beginning with the one which contains the smallest amount of water.

* This Journal, III, xvi, 42, July, 1878.

I. *Penig, Saxony.*

	I.	II.	Mean.	Relative number of atoms.			
P_2O_5	48·35	48·13	48·24	P ·678		1·	
Al_2O_3	33·50	33·60	33·55	Al ·651		·96	
Li_2O	8·97	8·97	8·97	Li ·598	·664	·98	
Na_2O	2·06	2·03	2·04	Na ·066			
Mn_2O_3	·12	·15	·13				
H_2O	1·75		1·75	OH ·194	·786	1·16	
F	11·26		11·26	F ·592			
			105·94				
O equivalent of F			4·74				
			101·20				

II. *Montebras, France, variety A.* G.=3·088.

	I.	II.	Mean.	Relative number of atoms.			
P_2O_5	47·10	47·07	47·09	P ·664		1·	
Al_2O_3	33·20	33·25	33·22	Al ·646		·97	
Li_2O	7·93	7·90	7·92	Li ·528			
Na_2O	3·48	....	3·48	Na ·112	·649	·98	
CaO	·25	·24	·24	Ca ·009			
H_2O	2·25	2·29	2·27	OH ·252	·775	1·17	
F	10·00	9·86	9·93	F ·523			
			104·15				
O equivalent of F			4·02				
			100·13				

III. *Auburn, Maine.* G.=3·059.

	I.	II.	Mean.	Relative number of atoms.			
P_2O_5	48·56	48·40	48·48	P ·683		1·	
Al_2O_3	33·67	33·90	33·78	Al ·656		·96	
Li_2O	9·49	9·42	9·46	Li ·630	·662	·97	
Na_2O	·96	1·02	·99	Na ·032			
H_2O	3·61	3·52	3·57	OH ·396	·722	1·06	
F	6·26	6·15	6·20	F ·326			
			102·48				
O equivalent of F			2·61				
			99·87				

IV. *Hebron, Maine, variety A.*

		Relative number of atoms.			
P_2O_5	By difference [48·53]	P ·682		1·	
Al_2O_3	34·12	Al ·662		·97	
Li_2O	9·54	Li ·636	·646	·95	
Na_2O	·34	Na ·010			
H_2O	4·44	OH·493	·769	1·13	
F	5·24	F ·276			

This sample was accidentally lost before a phosphoric acid determination was made. It is inserted because it is regarded as a good analysis and it varies somewhat from the other sample from Hebron which was obtained to replace it.

V. *Paris, Maine.* G.=3·035.

	I.	II.	Mean.	Relative number of atoms.			
P_2O_5	48·28	48·35	48·31	P	·680		1·
Al_2O_3	33·87	33·50	33·68	Al	·654		·96
Li_2O	9·83	9·80	9·82	Li	·654	·664	·97
Na_2O	·43	·24	·34	Na	·010		
K_2O	·03	...	·03				
H_2O	4·96	4·82	4·89	OH	·543	·797	1·17
F	4·82	4·82	4·82	F	·254		
			101·89				
O equivalent of F			2·03				
			99·86				

VI. *Hebron, Maine, variety B.* G.=3·032.

	I.	II.	Mean.	Relative number of atoms.			
P_2O_5	47·44	47·44	47·44	P	·668		1·
Al_2O_3	33·79	34·01	33·90	Al	·658		·98
Li_2O	....	9·24	9·24	Li	·616	·638	·95
Na_2O	....	·66	·66	Na	·022		
H_2O	5·00	5·10	5·05	OH	·561	·848	1·27
F	5·53	5·36	5·45	F	·287		
			101·74				
O equivalent of F			2·29				
			99·45				

VII. *Branchville, Connecticut.* G.=3·032.

	I.	II.	Mean.	Relative number of atoms.			
P_2O_5	48·80	48·81	48·80	P	·686		1·
Al_2O_3		34·26	34·26	Al	·665		·97
Li_2O	9·69	9·90	9·80	Li	·653	·659	·96
Na_2O	·15	·24	·19	Na	·006		
Fe_2O_3	·29	·29	·29				
Mn_2O_3	·10	·10	·10				
H_2O	5·93	5·90	5·91	OH	·658	·750	1·09
F	1·75	1·75	1·75	F	·092		
			101·10				
O equivalent of F			·74				
			100·36				

VIII. *Montebras, France, variety B.* G.=3·007.

	I.	II.	Mean.	Relative number of atoms.			
P_2O_5	48·31	48·38	48·34	P	·681		1·
Al_2O_3	33·73	33·38	33·55	Al	·651		·96
Li_2O	9·53	9·50	9·52	Li	·634		
Na_2O	·34	·33	·33	Na	·010	·654	·96
CaO	·40	·30	·35	Ca	·010		
H_2O	6·61	6·61	6·61	OH	·734	·826	1·21
F	1·76	1·74	1·75	F	·092		
			100·45				
O equivalent of F			·74				
			99·71				

For more easy comparison the ratios from the above analyses are collected in the following table by themselves, where $\dot{R}$ equals Li and Na.

		P	Al	$\dot{R}$	(OH, F)
I.	Penig, Saxony	1·00	·96	·98	1·16
II.	Montebras, France, A	1·00	·97	·98	1·17
III.	Auburn, Maine	1·00	·96	·97	1·06
IV.	Hebron, Maine, A	1·00	·97	·95	1·13
V.	Paris, Maine	1·00	·96	·97	1·17
VI.	Hebron, Maine, B	1·00	·98	·95	1·27
VII.	Branchville, Conn.	1·00	·97	·96	1·09
VIII.	Montebras, France, B	1·00	·96	·96	1·21

It will be seen that all of these approach closely to the ratio 1 : 1 : 1 : 1, hence I propose the formula $Al_2P_2O_8+2R\,(OH, F)$ or $\left.\begin{matrix}3Al_2P_2O_8\\ 2R_3PO_4\end{matrix}\right\} + \left\{\begin{matrix}Al_2\,(OH, F)_6\\ 2R\,(OH, F)\end{matrix}\right.$ as the true formula for all varieties of this mineral.

DesCloizeaux, from a difference in optical characters made out by him, has divided the mineral into two species: the original amblygonite, including I and II in the above list; and a second species for which he proposed the name *montebrasite* (hebronite of von Kobell), including analyses III to VIII above. The mineral from Branchville has not been examined optically and the material is very unfavorable for such an examination. Owing to the close identity in chemical composition it seems that a slight variation in optical properties is hardly sufficient ground for dividing the mineral into two species, but on the contrary I think that the old name amblygonite should be retained, and that all varieties should be included by it.

It will be seen in comparing the above ratios that in every case the ratio of P to (OH, F) is in excess of that of the Al to R. Two theories suggest themselves to account for this, the first of which seems the most plausible. First: most minerals which are ordinarily regarded as anhydrous contain a small amount of water, which is not calculated in the ratios, and which is not regarded as essential to the composition. Now if these minerals contain a small quantity of such accidental water, it will bring up the ratios very considerably, owing to the small molecular weight of water, and if the slight variation between P, Al and R be regarded as due to error of analysis the excess of (OH, F) would be easily accounted for. Second: if we regard the difference between P, Al and R as not due to error of analysis, and the fact that the variation in all is so constant suggests this, and regard enough of the water basic so that when added to the Al and R it will make the ratio with P equal 1 : 1 : 1, then the ratio of (OH, F) will be: Penig, 1·02;

Montebras, A, 1·06: Auburn, 0·91; Hebron, A, 0·99; Paris, 1·02; Hebron, B, 1·18; Branchville, 0·97; Montebras, B, 1·05. This relation seems rather striking, and although it is not as simple as we should like to have it, or perhaps as plausible as the first theory, yet it may possibly be the correct one. Whichever of these explanations is accepted, it will not materially alter the formula above made out for the minerals, the variation from which is too slight and not constant enough to be expressed by any different formula. It will be seen from analyses I and II that water is found in the Penig and Montebras varieties which have been regarded as anhydrous by some analysts. This may have been overlooked, and it is worth noting that in Plattner's Blowpipe Analysis the statement is made, of the Penig mineral, that water is expelled by heating in a closed tube. It will also be seen that these analyses differ from the older ones in that they are lower in alumina and higher in alkalies. I have thought it best to give my method of analysis in full, which may account for some of the variations.

Method of Analysis.

Water was determined by ignition with oxide of lead in a porcelain crucible; it is completely driven off only by strong ignition, and it was found necessary to fuse the contents of the crucible over the blast lamp before constant results could be obtained. Fluorine was determined by decomposing a mixture of the mineral and powdered quartz with sulphuric acid, converting the silicon fluoride formed into hydrofluosilicic acid, precipitating the hydrofluosilicic acid with potassium chloride and titrating the liberated hydrochloric acid with a standard alkali solution.* The varieties from Penig and Montebras are decomposed only by prolonged action of sulphuric acid. Phosphoric acid was determined by fusing the mineral with sodium carbonate, boiling out the fused mass with water and dilute nitric acid, nearly neutralizing the excess of acid with ammonia, precipitating with molybdic solution and then proceeding in the usual way.

To determine the bases, one gram was weighed into a large platinum crucible, mixed into a paste with from two to three cubic centimeters of sulphuric acid, and heated, with the crucible covered, over a low gas flame till not over a cubic centimeter of sulphuric acid remained. The contents of the crucible were then rinsed into a platinum evaporating dish and treated with a quantity of strong hydrochloric acid; after heating and concentrating a clear solution was obtained. The excess of acid being removed by evaporation, the contents of the dish were rinsed into a beaker, filtered where necessary, the

* See Remsen's American Chemical Journal, vol. i, No. 1.

undissolved portion after incinerating the filter paper was treated with hydrofluoric acid, then with a drop of sulphuric acid; the hydrofluoric acid expelled by evaporation and the solution added to the other solution of the bases. To obtain the bases as chlorides the sulphuric acid was precipitated from the solution with barium chloride, and the barium sulphate filtered off. The solution was then heated to boiling and a hot solution of barium hydroxide added; this precipitated all the phosphoric acid and part of the alumina. The solution contained all the lithia and most of the alumina which went into solution in the excess of barium hydroxide. After filtering and washing the precipitate it was dissolved in hydrochloric acid, the excess expelled by evaporation; the residue taken up in a few drops of hydrochloric acid and water and poured when hot into a boiling solution of sodium hydroxide and a little barium hydroxide in a platinum dish; this again precipitated all the phosphoric acid while the alumina went into solution in the alkaline hydroxides. After filtering, the filtrate was acidified with hydrochloric acid, the barium precipitated with sulphuric acid, and the alumina precipitated with ammonia. The alumina found at this point amounted usually to about one-half per cent. The insoluble barium phosphate containing also traces of iron, manganese, and calcium was dissolved in hydrochloric acid, the barium precipitated with sulphuric acid, filtered and the filtrate made alkaline with ammonia; this precipitated any iron, manganese or calcium as phosphate which was filtered off and examined separately. It was intended at this point to determine the phosphoric acid by direct precipitation with magnesia mixture, but the results coming out too low an examination of all the barium sulphate precipitates showed that a quantity of the phosphoric acid had been precipitated along with the barium sulphate. The first filtrate from the barium hydroxide precipitate contained, free from phosphoric acid, all the lithia, the larger part of the alumina, and the excess of barium. It was heated to boiling and ammonium carbonate added; this precipitated the barium and aluminium and left the lithia in solution. The precipitate was washed first by decantation, then with hot water on the filter pump; it was not considered free from lithia, however, as it is practically impossible to wash a large barium carbonate precipitate free from lithia. The filtrate was evaporated to dryness, the ammonia salts expelled by ignition, traces of barium separated a second or third time when necessary, evaporated to dryness again, lithia separated from soda and potash by means of absolute alcohol and ether, the lithia weighed as sulphate and the soda and potash as chlorides. The chlorides were tested carefully for potash by evaporating with excess of platinum

chloride and taking up in alcohol. The precipitate produced by ammonium carbonate was dissolved in hydrochloric acid, the barium precipitated with sulphuric acid, filtered and alumina precipitated in the filtrate with ammonia, the precipitate was washed with hot water, ignited finally over the blast lamp to expel sulphuric acid and weighed as oxide. The filtrate from the alumina was regarded as containing a trace of lithia which had been retained by the barium carbonate precipitate, it was evaporated to dryness, the ammonia salts expelled by ignition, taken up in water, filtered into a weighed crucible, evaporated to dryness and weighed, the lithia found amounted to from one-quarter to one per cent.

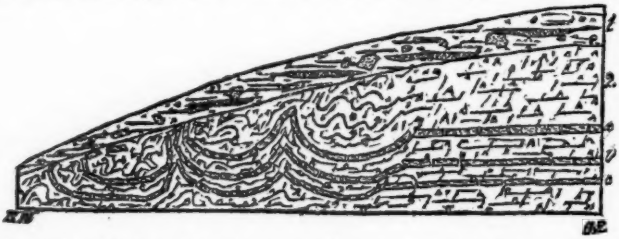
The solutions were kept as far as possible from all contact with glass, the evaporations being carried on in large platinum dishes. The reagents were carefully selected and purified. Sodium hydroxide free from aluminium and silica was obtained, prepared from metallic sodium. Owing to the limited amount of material from Penig only three-quarters of a gram was used in the determinations, and duplicates of the water and fluorine determination were not obtained. For the occurrence and associations of amblygonite at Branchville, Connecticut, see the papers by Messrs. Brush and Dana.*

In closing I wish to acknowledge my indebtedness to Professor Geo. J. Brush, who has most liberally furnished me with the material needed for this examination.

Sheffield Laboratory, June 18, 1879.

ART. XL.—*On the Superposition of Glacial Drift upon Residuary Clays;* by W. J. McGEE.

THE accompanying actual section is exposed in a cut on the Delaware & St. Paul Railroad, a mile north of Delaware, Delaware County, Iowa. No. 1 is glacial drift, somewhat light



and sandy, but containing erratics and continuous with the mantle of "ground moraine" deposits covering the greater por-

* This Journal, July and August, 1878, and May, 1879.

tion of the State. A few fragments of chert from the underlying Niagara Limestone are scattered through the deposit. Its thickness in this section varies from one to three feet, but in the southeasterly end of the same cut it is considerably thicker. No. 2 is red clay with nodules of chert, but without sand or erratics. In no respect is it distinguishable from the residuary clays of the Wisconsin driftless region fifty miles to the eastward. Three unusually regular and uniform layers of chert are exposed, yet remaining *in situ* through the greater part of the length of the cut. They are represented at *a*, *b*, and *c*. To the northwesterly end of the cut, however, they are contorted as represented, evidently by glacial action. Still greater contortion is exhibited in the lines of original stratification which can occasionally be detected in the enveloping clay. These contortions must have strongly disturbed and corrugated the adjacent surfaces; but all trace of this was subsequently removed by the glacier.

Though these members are so diverse in character, there is no well-defined plane of contact between them, nor is the surface of No. 2 in any place, so far as observed, smoothed or striated.

The direction of glacial motion here, as determined by the position of neighboring asar, was S. 50° or 55° E. The elevation of the section is 540 feet above the Mississippi at Dubuque.

Several analogous sections have been exposed in excavating wells two miles north of Farley, Dubuque County, Iowa, and in a cut on the Dubuque & Southwestern Railroad, half a mile southwest of the same place. Here, however, the drift is thicker and more compact than in the Delaware section, and contains a greater number of erratics; and many silicified Niagara fossils are found in the residuary clay, both free and imbedded in the nodules of chert. Aside from the thickness of the overlying beds of glacial drift we have here no positive evidence, such as is afforded by the Delaware section, that the residuary clay may not have been formed since the glacial period. Its surface has not been found to be either smoothed or furrowed.

Near Rockford, Floyd County, Iowa, there is an extensive exposure of the upper beds of the *Hamilton Limestone and Shales*,* here consisting of stiff blue and buff clays. It is the opinion of Prof. Calvin, of the Iowa State University, who has studied the formation at many exposures, that these clays were never much more firmly indurated than at present.

A section here exposed is as follows:—

1. Drift, with large bowlders,.....1 to 4 feet.
2. Clays of the Hamilton shales, about .....50 feet.

* Sometimes denominated the *Rockford shales*. For the relations of the formation, with a description of some of its fossils, see Prof. S. Calvin's papers in this Journal, vol. xv, p. 460, and in Bull. Geol. and Geog. Surv. Terr., vol. iv, No. 3, p. 325.

The ice here moved S. 20° or 30° E., and must have been of immense thickness, as attested by the presence of northern bowlders 20 to 40 feet in diameter within a few miles to the leeward; yet the subjacent clays were not seriously disturbed, as evidenced by numerous absolutely perfect fossil Devonian brachiopods in the clay. The ice, too, must have ascended obliquely the steep slope of the bluff. A ground and striated specimen of *Orthis Vanuxemi*, and an *O. Iowensis* with a smoothed plane surface on the dorsal valve picked up from the talus at the base of the section, would indicate that the surface of the clays might exhibit glaciation on fresh exposure. At the time of examination the section was much weathered.

In none of these sections, nor in any of the many others which might be given, did space permit, are the pre-glacial clays more compact than is the lower till or blue clay found immediately below the latest formed glacial drift over the greater part of the State. It is this later drift which overlies the residuary and sedimentary clays in every instance.

The conclusions which a study of these and similar sections seem to justify may be briefly stated: (1) That residuary clays (and, by inference, other clays of equal compactness) were passed over by a thick ice-sheet (*a*) in some instances without removal or even serious disturbance, while (*b*) in other cases they were removed, or contorted and broken up, just as the lower till and associated deposits have been found to be on both sides of the Atlantic;* and (2) that the plane of contact between glacial drift and subjacent beds of residuary clay (and, by inference, other clays of similar consistency) is not necessarily clearly defined—the materials so intermingling as to form a thin intermediate stratum of composite character. Analogy with the many recorded instances in which the upper surface of the lower till is scored and smoothed† and with the "striated pavements" of Hugh Miller, would, however, seem to justify the assumption that residuary clays may sometimes be smoothed and furrowed.

Farley, Iowa, July 30, 1879.

* See Lyell, "Student's Elements," 1871, p. 179, and Antiq. Man, 1873, p. 262; Croll, "Climate and Time," Am. Ed., p. 465; Geikie, "Great Ice Age," Am. Ed., pp. 144–5; Chamberlin, Geol. Wis., 1877, vol. ii, p. 219; Foster and Whitney, Geol. Lake Superior Dist., 1851, pt. ii, p. 245; Logan, Geol. Can., 1863, pp. 898, 902, 906; Reid, Geol. Mag., vol. vi., p. 379; LeConte, this Journal, vol. xviii, p. 40; Hinde, Canadian Journal, April, 1877; McGee, Proc. Am. Assoc., 1878.

† Chamberlin, loc. cit., p. 226; Lyell, "Student's Elements," p. 178; Geikie, loc. cit., p. 151, and also Trans. Geol. Soc. Glasgow, 1863, vol. i, pt. ii. p. 68, and "Glacial Drift of Scotland," p. 67; Read, Geol. Ohio, 1878, vol. iii, pt. i, p. 312; Hinde, loc. cit. (striated pavements also occur near Toronto and are here described); Dawson, cited by Logan, loc. cit., p. 919; Reid, loc. cit.; Chambers', Edin. New Phil. Jour., vol. liv, p. 272; and Smith, "Newer Pliocene Geology," p. 129. The last two authorities are cited by Croll, loc. cit., p. 256.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Theory of Fractional Distillation.*—Thorpe has given a striking instance of the general law announced by Wanklyn and confirmed by Carey Lea and Berthelot, that when two liquids of different boiling points are mixed together in equal quantities by weight, and subjected to distillation, the quantity of each constituent in the distillate is proportional to the product of its vapor-density and vapor-tension at the temperature of ebullition of the mixture; and hence that when the vapor-tensions of the two liquids are inversely proportional to their vapor-densities, the liquid will distill unchanged. Berthelot, for example, observed that a mixture of 90·9 parts carbon disulphide, boiling at 46·6°, vapor-density 38, and 9·1 parts ethyl alcohol boiling at 78·4, vapor-density 23, behaved on distillation like a homogeneous liquid; the ratios obtained by multiplying the vapor-tensions and densities being 88·5 and 11·5. Thorpe's results were obtained with a mixture of equal volumes carbon tetrachloride, boiling point 76·6°, vapor-density 76·7, and methyl alcohol, boiling point 65·2°, vapor-density 15·97; and he noticed that 46·5 per cent of the whole boiled constantly between 55·6° and 55·9°, or nearly 10° lower than the boiling point of the most volatile constituent. By vapor density determinations, the composition of the mixture by weight was found to be 78·1 CCl_4 and 21·9 CH_4O; a ratio almost identical with that obtained by multiplying the vapor-tensions of the two liquids at the temperature of the boiling point of the fractions (55·7°) by their respective vapor densities. Hence a mixture of 1 part methyl alcohol and 3·6 parts carbon tetrachloride, boils like a homogeneous liquid at about 10° lower than the boiling point of the methyl alcohol, the more volatile of the two liquids. On continuing the distillation of the liquid remaining in the flask, the several fractions gave numbers showing clearly that the CCl_4, although having the higher boiling point, passed over in largest quantity in the first fractions, the quantity of pure methyl alcohol increasing as the temperature rises. Thorpe suggests, as a lecture-experiment to show the effect of such a mixture, that into one of three barometer tubes over mercury, a few drops of methyl alcohol be put, an equal quantity of carbon tetrachloride being placed in the second, and a mixture of the two in the proportion of 3 c.c. of CH_4O and 5 c.c. of CCl_4 in the third. In the first tube the mercury will be depressed about 80 mm. and in the second 70 mm.; while in the third the depression will be 130 mm.—*J. Chem. Soc.*, xxxv, 544, Aug. 1879. G. F. B.

2. *On the Solidifying point of Bromine.*—Because of the widely differing values given for the point at which bromine solidifies, Philipp, at the suggestion of Rammelsberg, has undertaken to redetermine it. In order to purify the bromine, it was

dissolved in caustic baryta water, the washed barium bromate converted into bromide by ignition, and this distilled with sulphuric acid and potassium dichromate. The distillate was washed and dried; a part by agitation with concentrated sulphuric acid and distillation and a part by distillation from calcium chloride. Thus purified, the bromine solidified between $-7{\cdot}2°$ and $-7{\cdot}3°$, phenomena of surfusion not being noticed. This result corresponds with that obtained by Regnault $-7{\cdot}32°$. The non-purified bromine solidified at $-9°$ to $-10°$. To test the influence of foreign admixture, the author made direct experiments and found that while 2 per cent of iodine did not materially raise the solidifying point, 3 or 4 per cent of chlorine lowered it even to $-15°$. Solid bromine has a brown color and a conchoidal fracture; though after exposure to the air, it takes a gray color recalling that of iodine, and appears crystalline.—*Ber. Berl. Chem. Ges.*, xii, 1424, July, 1879. G. F. B.

3. *On the Thermic formation of Hydrogen silicide and of Ethyl silicate.*—Ogier has studied, in Berthelot's laboratory, the heat-relations attending the formation of hydrogen silicide and of ethyl silicate. The hydrogen silicide was prepared by the action of sodium on tribasic siliciformic ether, and was free from hydrogen. It was burned with oxygen in the small glass chamber of a water calorimeter, being ignited by a small induction spark. The quantity of gas used was determined from the increased weight of the chamber and of a tared eduction tube filled with fragments of pumice moistened with sulphuric acid. In this way the heat of combustion of one equivalent of SiH_4 was found to be 324·3 calories. From this, since Si (cryst.) $+O_4 = +211{\cdot}1$ cal. and $4(H+O) = +138$ cal., $SiH_4 + O_8 = +324{\cdot}3$ cal. as above, we have $Si+H_4 = +24{\cdot}8$ calories. Hence in uniting Si and H_4 evolve 24·8 calories, a number very near that which the formation of marsh gas gives, +22.

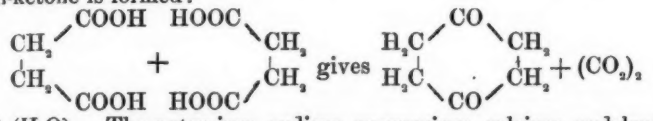
The heat of formation of silicic ether was determined in two ways: first, analytically, by decomposing it by means of a large quantity of water, into silicic acid and alcohol, at the ordinary temperature; and second, synthetically, by forming it directly by the action of silicium chloride upon absolute alcohol. The former method gave $+21{\cdot}6$ calories as the heat evolved in the decomposition. Taken with the contrary sign, it expresses the heat absorbed in the formation of the ether from the alcohol and silicic acid. Subtracting from this the heat corresponding to the solution of the alcohol in water, there is left $-11{\cdot}44$ calories, the true heat of formation of silicic ether. The latter method showed that the heat evolved when one equivalent silicon tetrachloride acted on 26 equivalents of alcohol was 42·3 calories at 10°. Making the necessary corrections, the heat of formation of silicic ether obtained in this way, is $-11{\cdot}56$ calories, a close accordance with the number obtained analytically, $-11{\cdot}44$. Referred to a single equivalent of alcohol instead of four, it becomes $-2{\cdot}9$.—*Bull. Soc. Chim.*, II, xxxii, 116, 118, Aug., 1879. G. F. B.

4. *On Organic Ultramarines.*—DEFORCRAND has succeeded in producing an ultramarine containing ethyl. Though Unger first succeeded in replacing half of the sodium in ultramarine by silver, Heumann first produced an ultramarine in which this replacement was complete. This yellow silver-ultramarine served as the starting point in the new researches. When heated, dry, with a metallic or an organic chloride, silver chloride and a new ultramarine result. Silver ultramarine was prepared by heating in sealed tubes for 15 to 16 hours, four or five grams of blue ultramarine and 8 or 10 grams silver nitrate in concentrated solution. A beautiful yellow powder in transparent grains is thus obtained, which contains 46 to 47 per cent of silver, and has all the properties of an ultramarine. From it the blue ultramarine may be regenerated either by heating with a strong solution of sodium chloride for twenty-five hours, the reaction being then limited by the inverse one; or by heating the two without water to a high temperature, in which case the conversion is complete. If other metallic chlorides be used in place of sodium chloride, a series of ultramarines is obtained containing the metal used in place of the silver, there being a definite temperature for each at which its formation is a maximum. The potassium and rubidium ultramarines are greenish blue, that of lithium is blue, of barium yellowish-brown, of zinc violet, of magnesium gray, etc. Mercuric chloride gives a gray mercury-ultramarine when heated directly with the blue sodium ultramarine. Heated with ethyl iodide in a sealed tube to 180° for from fifty to sixty hours, and repeating the operation several times, silver ultramarine is decomposed, yielding a clear gray powder with a reddish cast, which even at 100° evolves ethyl sulphide. If, however, this powder be intimately mixed with sodium chloride before heating it, only a trifling evolution of ethyl sulphide takes place, and the mixture becomes blue owing to the regeneration of sodium ultramarine. The reaction is complete at the temperature at which the sodium chloride melts, the ethyl being evolved as chloride. That the gray powder actually contained ethyl was proved by heating a gram of it to redness in a tube through which an inert gas passed, the products being collected in a solution of mercuric chloride. A white crystalline precipitate of $(C_2H_5)_2S.HgCl_2$ was formed, thus proving the gray powder to be a true ethyl ultramarine. Similar compounds were obtained with allyl, amyl and benzyl, and with the compound ammoniums.—*Ann. Chim. Phys.*, V, xvii, 559, Aug., 1879.

G. F. B.

5. *On the Synthesis of the Benzene Ring.*—Perhaps no hypothesis in science has been more fruitful of results than the classic one of Kekulé in relation to benzene, which supposes that its six carbon atoms form a closed ring. Since the few synthetic methods, by which this ring has until now been formed, do not decide between this and the so-called prismatic arrangement of the benzene nucleus, VON RICHTER has sought for and discovered a direct synthesis of the benzene ring which strongly confirms the hypoth-

esis of Kekulé. Exactly as monobasic acids yield common ketones, so the diabasic acids should give double ketones having the carbon atoms in a ring form. Thus from succinic acid, di-ethylene-di-ketone is formed:



$+(H_2O)_2$. The potassium, sodium, magnesium, calcium and lead salts of succinic acid were submitted to distillation under various conditions. A dark colored oil in greater or less quantity was always obtained, having a ketone-like odor, but from which no fractions of exact boiling point were obtainable. By distilling the fraction boiling between 160° and 250° with zinc dust, considerable benzene was obtained. Moreover hydroquinone was contained in the wash waters of the crude oily distillate. Since the above di-ketone yields both these bodies readily, $C_6H_8O_2=C_6H_6O_2+H_2$ and $C_6H_8O_2+Zn_2=C_6H_6+(ZnO)_2+H_2$, it may be considered as proved that the benzene nucleus has the constitution assigned to it by Kekulé. Moreover this experiment fixes hydroquinone as a para-compound and establishes the quinones as double ketones.—*J. pr. Ch.*, II, xx, 205, Aug., 1879. G. F. B.

6. *On the Sulpho-ethers of the Polyatomic Alcohols and the Carbohydrates.*—Claesson showed, a short time ago, that chlor-sulphuric acid, $SO_2\begin{cases}OH\\Cl\end{cases}$, acted on the monatomic alcohols to form mono- or di-sulphuric ethers. He has now extended the reaction to the polyatomic alcohols and has obtained from glycol, ethylene disulphate, from glycerin, glyceryl trisulphate, from erythrite and mannite, tetra- and hexa-sulphates respectively, and from dulcite, a penta-sulphate. The carbohydrates of the glucose group give by this treatment, isomeric compounds probably monochlor-tetrasulphates. At least this is the case with dextrose whose derivative is crystallizable and has the composition $CH_2OSO_2OH.(CHOSO_2OH)_3.CHCl.CHO$. Cane sugar, starch, etc., are first inverted and then the above compounds are formed. These polysulphates of the optically active alcohols and carbohydrates have an increased rotatory power to the right. Milk sugar gives dextrose and galactose.—*J. pr. Ch.*, II, xx, 1, Aug., 1879.

G. F. B.

7. *On the Conversion of Aurin into Trimethyl-pararosaniline.*—Dale and Schorlemmer, by acting on aurin with ammonia, have sought to obtain the intermediate products between this substance and para-rosaniline. As ammonia gave so much trouble, they tried methylamine and found that in aqueous solution, this base acts readily on aurin at 125° and transforms it almost entirely into a purple body, possessing all the properties of a trimethyl-rosaniline: $C_{19}H_{14}O_3+(CH_3NH_2)_3=C_{19}H_{14}(CH_3)_3N_3+(H_2O)_3$. Trimethylamine acts similarly, converting the aurin into purple coloring matters.—*J. Chem. Soc.*, xxxv, 562, Aug., 1879.

G. F. B.

8. *An Induction Balance.*—Professor J. E. Hughes has lately devised an instrument which promises to be of great use in determining the amount of the constituents of alloys. It is based upon the principle that the induced current in a secondary coil depends upon the character and amount of metal which forms the core of the primary. A portion of the apparatus Professor Hughes calls a sonometer; this consists merely of two primary coils which are placed with opposing poles about a meter apart on a divided scale. Between them slips a secondary coil which is connected with a telephone. If the primary coils are exactly equal, and are traversed by the same electric current, one will hear no sound when the secondary coil is exactly between the primary coils. This point of balance is called the zero of the sonometer. The circuit running through the primary coils is provided with a microphone, and a clock ticking upon the support of the microphone supplies the necessary change of resistance in the electric circuit. If the equality of the sides of the sonometer is disturbed by the introduction of metals on one side or the other, the telephone announces the inequality and the secondary coil has to be moved nearer one primary coil than another. The number of degrees moved is a relative measure of the difference of the metals. The extreme sensitiveness of this balance is shown by numerous experiments. It is also of use as a coin detector—any difference in the quantity or quality of the metal being instantly shown. W. Chandler Roberts, Chemist of the Mint, has tested Professor Hughes' balance, and gives a number of curves produced by different alloys, and shows that the balance can detect smaller quantities of metals in the composition of alloys than the methods hitherto used. He suggests also "that the balance may afford a simple means of detecting variations in the molecular structure of alloys and for detecting allotropy in metals with greater accuracy than has hitherto been possible."—*Phil. Mag.*, July, 1879, p. 50. J. T.

II. Geology and Natural History.

1. *Notice of Volcanic Phenomena and Earthquakes during* 1878.—The statistical review of these phenomena recently published by Professor C. W. C. Fuchs shows the unusually large number of *twelve* eruptions during the year; most of which occurred in remote localities and from little known volcanoes. In Vesuvius there was but slight activity, with a small flow of lava in September and November. On January 10, smoke was seen from two hitherto unknown volcanoes at the southern point of South America. On the same day a great eruption occurred in the island of Tanna, one of the New Hebrides, followed by a second outbreak on February 4. Simultaneously yet another eruption occurred in the island of Birara in the group of New Britain. Another eruption took place in February from the volcano Isluga in South America (lat. 19° 10′ S.), where several villages were destroyed by the lava streams and accompanying

earthquake. Other eruptions were from Mount Hecla (March), from the Asamayama in Japan, from Cotopaxi (October), from the Tepaco, the Sitna, and the Isalco in San Salvador, from the volcanoes of the Aleutian Islands and in the Society Islands. Dr. Fuchs also records the great mud eruption near Paterno in Sicily, which began on Dec. 10, and still continued at the end of the year.

The number of earthquakes reported during 1878 amounts to 103. But among these are many complete earthquake periods during which the shocks continued with short intervals for hours, days or even weeks in the same locality. If every shock were counted the total would be many times greater.

The earthquakes were most frequent in winter and autumn—thirty-nine occurring in winter, twenty-six in autumn, and nineteen each in summer and spring. The most violent and destructive earthquakes occurred on January 23 in Peru and Boilvia, and on October 2 in San Salvador. (Also on April 12 in Venezuela. This Journal, Feb., 1879, pp. 158, 159, 161.) Of European earthquakes the following deserve notice. On January 28, about noon an earthquake occurred in the northwestern part of France and the south of England, particularly distinct in Normandy. Repeated shocks were felt in northwestern Switzerland and the southwest corner of the Black Forest on January 16, 17 and March 29. Other instances of repeated earthquakes are Innsbruck (Jan. 3, 10, 11, Feb. 2, Aug. 9), Gross Geran (Jan. 2, March 25), Lisbon (Jan. 26, 27, June 8), Constantinople and vicinity (April 19 to end of May). Less remarkable for its violence than for its extent was the Low-Rhenish earthquake of Aug. 26. The observations in this case were unusually exact and numerous, which gives additional interest. It began about 9 A. M., and was best observed in Cologne. Here it consisted of an undulatory rising and sinking of the ground, which increased in intensity to an alarming extent. On the cathedral tower the smaller bell struck several times, and in many places the houses showed cracks. At the end of the oscillations a dull subterranean noise was heard, and a second shock was observed by many persons. At other places the phenomena were similar. The area affected by the first shock may have measured over 2000 geographical square miles, as its outlines may be indicated as follows: Arnsberg and Hanover on the north, Offenbach on the Main and Michelstadt in the Odenwald on the southeast, Strasburg, Paris and Charelville in the south, Liége and Brussels in the west, and Utrecht in the northwest.

From collating the most reliable observations of time Professor Klinkerfues infers that the velocity of the earthquake in the ground was 6·78 geographical miles per second, and that its origin was between 6·3 and 8·7 miles below the surface.

It is remarkable that the phenomenon was only noticed at the surface and was more intense the higher the observer was above the ground, while miners working at a depth of 300 meters did not feel it at all.

A similar fact has been noticed in regard to some recent earthquakes in our Rocky Mountain region, which, though quite severe on the surface were not felt in the mines below.— *Condensed from Nature*, Aug. 14, 1879. C. G. R.

2. *The Geology of the Diamantiferous Region of the Province of Paraná, Brazil;* by ORVILLE A. DERBY, M.S. (Proc. American Phil. Soc., May 16, 1879.)—This paper records the results of a recent trip by Mr. Derby, to the Province of Paraná, in continuation of the labors of the late Geological Commission in the same region; and it also gives us for the first time an accurate idea of the relations of the deposits of extreme southern Brazil to those of the other parts of the Empire. The Province of Paraná, one of the more southern ones of Brazil, lies between the provinces of Sâo Paulo and Santa Catharina, and reaches from the Atlantic to the Rio Paraná. In its topographical and geological features it resembles, to a certain extent, the two provinces which border it on the north and south. The coast range of mountains, or Serra do Mar, traverses it in a north-south direction, leaving along the coast a low belt, from ten to twenty miles broad. The remainder of the province is, strictly speaking, a plateau, from 800 to 1000 meters high; but Mr. Derby divided the entire province into two distinct topographical regions, a mountainous region, bordering the coast and extending inland about 100 miles, and a plateau region, occupying the central and western parts of the province. The first region is entirely composed of metamorphic rocks, highly inclined and with a general strike east-northeast. These, in the coast belt, and in the Serra do Mar proper, are mostly granites and gneisses, representing the Archean of Rio de Janeiro and northern Brazil; but further west they consist principally of metamorphic schists, quartzites, marbles, etc., and represent the Lower Silurian or Cambrian of Bahia, Minas Geraes and northern Brazil. A second geological province extends from the metamorphic westward, a hundred miles or more, forming the far-famed "Campos Geraes," and made up of shales and coarse and fine sandstones. In the shales of this group at Ponta Grossa, were discovered species of *Lingula*, *Discina*, *Spirifera*, *Rhynchonella*, *Streptorhynchus* and *Vitulina*, strongly resembling, and probably identical with, those of the Devonian of the Amazonas. Other fossils were also obtained. The entire western part of the province is apparently formed of a heavy bed or series of beds of trap, overlying a considerable thickness of soft red sandstone, which, in turn, overlies the shales and sandstones of the second region. The rocks of the third region resemble in a striking manner the Triassic rocks of North America.

The diamonds are found principally in the valley of the river Tibagy, and more rarely in other river valleys, as they cross the second or Devonian area, above defined. The observations made tend to prove that the sand and gravel containing the diamonds were derived from the underlying Devonian sandstones, which had previously obtained their material from the lower-lying meta-

morphic series. The gems are found in the sands of the river, and in more elevated gravel banks, called "dry washings." R. R.

3. *Serpentine Marble.*—A beautiful variety of mottled serpentine marble is worked at a point on Broad Creek in Harford county, on lands of the Havre Iron Co., Maryland. Dr. Genth states that there are three beds of serpentine, associated with chloritic and other magnesian rocks in the mica schist formation of the region. The chief bed is about 500 feet thick, and is traceable by its outcrop for about 1500 feet.

4. *The Gymnospermy of Coniferæ;* by Dr. L. CELAKOVSKY. A paper in *Flora* for June, 1879, Nos. 17 and 18.—Celakovsky, who takes a high position as a morphological botanist, mentions that in the year 1874 he published in *Flora* an article opposing gymnospermy. He now announces that he has changed his opinion, having satisfied himself of the truth of this doctrine. The agent of conversion was a monstrosity of the Norway Spruce cone, like that from which Stengel made out the now accepted morphology of the cone, and the same monstrosity as that which Braun studied in the Larch, deducing from it the accepted doctrine many years ago. The essential point in this monstrosity is that the bracts of the abnormal catkin develop into leaves, and the carpellary scale before it into a pair of leaves transverse to the bract. The abietinous carpel consists of these two leaves united by their posterior edges (i. e., those next the axis of the cone) into a scale, the back of which therefore faces the axis of the cone, and bears the ovules. The lower part of these catkins is usually normal, the apex by prolification is gradually transformed in the manner here specified, and becomes a leafy branch. Dr. Engelmann, in this Journal, three years ago, gave a confirmatory account of an analogous monstrosity in the Hemlock Spruce, but in which the transformation was at the base of the cone, the lower bracts leaf-like and with a pair of leaves in their axil, the following bracts more and more scale-like, the geminate leaves in their axil were partially united, next forming a scale with a cleft or notched apex, then an entire carpellary scale, in the axil of a normal bract.

Celakovsky, having now seen the Spruce monstrosity for himself, adopts the inevitable conclusion, and applies it well to the settling of the question of gymnospermy. He declares that the dorsal origin of the ovules of the Abietineæ proves that it is no axillary production, and thus the main support of those who take the ovule for a simplified female flower falls to the ground. Moreover, the ovules of Coniferæ in retrograde metamorphosis never change into shoots, but simply disappear. If flowers, they would be expected sometimes to become foliaceous branchlets. So Celakovsky regards it as demonstrated that they are outgrowths from the dorsal face of the leaf, analogous to the sori and indusia of Ferns. He cites the indusium of *Hymenophyllum* as an instructive analogue, only it is marginal; that of *Davalia* is somewhat dorsal; that of *Cyathea* wholly so and yet cup-shaped. He goes on to say that the gymnospermy of *Abietineæ* being thus proved, that of the rest

of *Coniferæ* follows of course; that Braun has seen similar prolification in the catkins of *Taxodineæ*, in which the carpel-scale was replaced by a bud; that, although the carpel-scale in *Abietineæ* consists of two leaves, the bud may in other cases develope more than two leaves, so that the lobed scale of *Cryptomeria* may be composed of as many leaves as there are lobes. Moreover, although the ovules in *Abietineæ* originate from the scale, the greater part of the scale is developed after the formation of the ovules; and in *Cupressus* the scale is developed even as late as the following spring, while the ovules are produced in the autumn. However the case may be disguised, Celakovsky asserts his firm conviction, 1st, that an ovule can only be developed as depending on a carpel, and, 2d, that its nucleus represents the macrosporangium of vascular Cryptogams. He adds that this is the logical consequence of the theory of descent, and must be true if the doctrine of the genetic connection of the vegetable world is true. He considers that Van Tieghem and Strassburger have proved the seemingly simple scale of *Cupressineæ* and *Taxodineæ* to be composed of bract and carpel-scale united [which indeed is evident in *Taxodineæ*], and that Braun has confirmed this by the study of proliferous cones. As to the development of ovules earlier than the carpels they belong to, this is said to have been observed in some Angiosperms also, as in *Cuscuta*, in which at first four naked ovules appear. The anatomical organogenist may argue from this that ovules and carpels are independent productions, but Celakovsky insists that he will argue wrongly.

This brings our author to the consideration of the structure of *Taxineæ*. This is environed with difficulties, and explanation is only conjectural. Here the disc, arillus, cupula, or whatever it be called, makes its appearance where no trace of carpellary scale is to be seen. Celakovsky inclines to the view that this organ, occurring in whatever form, is most probably the carpellary scale itself, very tardily developed. In *Dacrydium* the cupule is homologous with that of *Taxus*, but oblique. *Cephalotaxus* has no scale and no cupule, but seems to correspond with *Cupressineæ*, and shows at maturity a small flattened rudiment between the two ovules, which is probably a rudimental carpel-scale. *Gingko* is the most puzzling; yet it seems probable that the biovuliferous peduncle represents the abietineous carpel-scale, the peduncle itself being its elongated base. The cupule of Taxus may be either a simple circular carpel, or may consist of more than one carpel. The apparently terminal ovule of *Taxus* and *Torreya* he would regard as axillary to one of the uppermost subtending bract-scales; for he will not concede that the ovule can be wholly destitute of a carpellary organ. Yet he might do so, in one sense; for if the carpel may develope very late and very imperfectly or very little, it may sometimes not visibly appear at all, and so the phyllome be reduced to the ovular outgrowth.

Finally, Celakovsky notes, that if the ovule of *Taxus* and *Torreya* be axillary to an uppermost scale, it would originate not from the dorsal but from the ventral face, i. e. from the upper side

of the leaf; which would distinguish *Taxineæ* from all true *Coniferæ*,—a view which would not be destitute of important support. For both Braun and Mohl have seen apparently androgynous scales in some *Abietineæ*. In a monstrous Larch-ament, among carpellary scales with normally dorsal ovules, Braun found one with ovules on the opposite face; and Mohl describes and figures an androgynous inflorescence of White Spruce, with pollen-sacs on the outer face, and on the other a pair of knobs which from their form and position might be taken for imperfectly developed ovules. But this latter case seems most ambiguous. If it was in a male catkin, the upper part of which had become female by the development of carpel-scales in the axil of stamens partially transformed into bracts (which is the case we have before us in a monstrosity of Hemlock Spruce), then the quasi-androgynous scale in question may have been the normal abietineous carpel-scale itself, with the polleniferous bract behind it and connate with it.

The androgynous spike of Hemlock Spruce before us is below normally staminate; above some anthers are slightly scarious-winged at one side of the projecting tip, another has this wing developed into a bract-like body on the whole of one side; next there is a bract with a single small pollen-sac on one side of its back and in its axil a well-formed and biovulate carpel-scale.

G. E. & A. G.

5. *Contributions to American Botany*, IX; by Sereno Watson. From *Proceedings of American Academy of Arts and Sciences*, vol. xiv, July, 1879, p. 213 to 303, and an index.—Mr. Watson, in preparing the Monocotyledoneæ for the Botany of California, came upon the order *Liliaceæ*, which is well represented in Pacific North America; and he had to consider how the genera and higher groups should be disposed. This led to a wide study of the order and a strict scrutiny of the American species; and the present "*Revision of the North American Liliaceæ*," occupying the greater part of the "Contribution" before us, is the result. It is generally agreed that this order is to have the wide extension which was given to it by the present writer a dozen and more years ago; and the proper collocation of its diversified forms, with interlaced affinities, has been a problem of no small difficulty. Mr. Baker, in England, has attempted the task for the order generally, and has sedulously elaborated some of the North American, but more of the Old World and the South American, genera and tribes. His arrangement and his systematic views are in many respects satisfactory, in some unsatisfactory as respects North American botany. Mr. Watson has the latter primarily in view, but still has to adjust the American genera into the general system. The arrangement he has planned consists of three *series*, the first of which parts into two *subseries*, and includes sixteen tribes, some of them divided into subtribes. The great endeavor has evidently been to make natural groups—and this endeavor has been really successful. The next thing is to assign characters, and here comes the difficulty. Absolute

characters of the leading groups are not to be had, even when North American forms only are considered. Those who imagine they could do better than Mr. Watson has done should make trial before they criticise. The character of the pericarp, whether baccate or capsular, the nature of the stock, whether bulbous, tuberous or rhizomatous, the nature of the seed-coat, the inflorescence, direction of anthers, union or separation of styles, are all good characters to a certain extent, and all fail to furnish unexceptionable marks to distinguish the higher groups when natural associations are sought. It is not easy to ascertain what diagnostic characters in this monograph are most to be trusted. But the nature of the bracts (on the one hand scarious, on the other foliaceous or none) takes the lead in the first two series, and is followed by the persistence or deciduousness of the perianth, the insertion of the stamens whether on the perianth or at its base, the dehiscence of capsule,—all matters of little physiological importance, but for that reason perhaps surer guides to affinity than the more prominent adaptive characters. However, it may be said that the first series answers to the *Asphodeleæ*, with *Yucca* and *Hemerocallideæ* added; the second to the true *Liliaceæ* with *Uvularieæ* and *Trillieæ* added; the third to *Melanthaceæ*, with the tribe *Tofieldieæ* appended. Thus disposed, it is doubtless judicious to designate the three primary groups as "series," and not as suborders, and to throw the stress upon the tribes.

The Melanthaceous series, which in our view best divides into the *Colchiceæ*, *Veratreæ* and *Tofieldieæ*—the first not American—is here divided into the *Veratreæ*, *Heloniеæ* and *Xerophylleæ* (which two we should combine), into the midst of which the *Tofieldieæ* are intercalated. This last tribe, which should end the series, is quite exceptional, and is well composed of *Tofieldia*, *Pleea* and *Narthecium*. Its marks are the equitant leaves, introrse anthers with parallel cells, and caudate seeds; but to bring *Narthecium* under the remaining character of "styles distinct or none," it is defined as destitute of style, but with "the slightly lobed stigma sessile upon the attenuated apex of the ovary." This is really much nearer the fact than would be supposed, as the cells of the ovary actually do taper up into the subulate style (as it has always and most naturally been termed), so that in the mature capsule the upper tails of the seeds reach up to within a short distance of the small stigma.

In a linear order it has not been practicable to approximate the *Convallarieæ* of the first series with the *Uvularieæ* of the second. The division of *Uvularia* gives a gratifying opportunity of dedicating a New England genus to the memory of one of the best of New England botanists, the late Wm. Oakes (*Oakesia sessilifolia*, with its relative of the southern mountains, *O. puberula*); but he would not have relished the dismemberment of the Linnæan genus upon the characters, good as they are, neither in fact do we. The formation of the tribe *Yuccеæ*, of *Yucca* and *Hesperaloe*, strikes us as excellent; and it seems right not to adopt the supposed second species of *Hesperaloe* until it is better known. Its

principal distinctions (longer anthers and shorter style) may indicate heterogone dimorphism, which would be a novelty in the order.

Tribe *Nolineæ*, of the first series, must be regarded as an excellent group, composed of *Dasylirion* and *Nolina;* and it is gratifying to find that the outlying genus, *Nolina*, founded long ago, on a single Georgian species, is the northern representative of a considerable Texano-Mexican group, named *Beaucarnea.* It were to be wished that the plan of this Revision had allowed more citation of generic synonymy, and that it had been more explicitly stated that *Beaucarnea* is only *Nolina.* This union, indeed, is one of the happy hits of the present monograph.

As has been suspected, the Californian *Schœnolirion album* of Durand proves to be quite distinct from the Atlantic species on which that genus was founded. So Mr. Watson has embraced the opportunity, here offered, to dedicate a peculiar Californian genus to Judge Hastings—a judicious patron both of botanical and legal learning. Except for his exertions, his own liberality and his direction of the liberality of others, we could not have had the Botany of California, which Mr. Watson may now soon bring to a completion. The reader finds no mention of this under the genus *Hastingsia*, p. 217, nor under the species, *H. alba*, p. 242. But an appropriate reference is made on p. 286.

Leucocrinum, Nutt., was conjectured by Endlicher to be the Mexican *Weldenia*, but it has just now been ascertained at Kew that *Weldenia* is a Commelynaceous genus.

Our species of *Allium* as now worked out by Mr. Watson with great painstaking, are thirty-six in number, exclusive of the introduced *A. vineale.* Some characters might be made more of in living plants, such especially as those furnished by the so-called "crests of the ovary." In *A. stellatum* these crests are remarkably developed, radiating from around the base of the style and recurving, the notch at the end of each fitting over the base of the alternate filaments, and the under side is nectariferous and attractive to bees. The flowers are proterandrous.

In separating the two species of *Maianthemum* we should have unhesitatingly referred the large Pacific coast form to *M. bifolium.* We should not have distinguished *Lilium Grayi* as more than a form of *L. Canadense*, one which extends northward to the central parts of New York. In view of geographical range, size, and general appearance, we should never have thought of *Uvularia flava* as a synonym of *U. grandiflora.* Mr. Watson finds good characters in the shape and markings of the capsule to separate *U. grandiflora* from *U. perfoliata.* Has any one ripe fruit of the small, yellow-flowered, *U. flava?*

Chamælirium Carolinanum, Willd. This specific name is properly restored. It was the original name under this genus; and the name *luteum* is a false one (though the plant was *Veratrum luteum* of Linnæus), the blossoms being white without a tinge of yellow, duller white in the female plant, pure white in the male, the pedicels equally of this color.

No space is left in which to notice the Notes upon the Affinities and geographical Distribution of *Liliaceæ*, nor the Descriptions of Some new Species of North American Plants, about fifty in number, which make up the second part of this important "Contribution." Among them is a new *Bolandra* and a new *Sullivantia* from Oregon, both very much like (we fear too like) the original species. Here and in the Bibliographical Index, the name *Sullivantia Ohionis* is changed (perhaps accidentally) to *S. Ohioensis*. We know of no law against genetive names of geographical more than of other places or stations, and such are not extremely uncommon. The name *Ohionis* was purposely chosen, and we hope may be retained.

The interesting new Erigoneous genus *Hollisteria*, discovered by the enthusiastic Mr. Lemmon (in San Luis Obispo Co., east of the Coast Range) is of rather doubtful interpretation as to some points of structure. The inflorescence we suppose to be only seemingly axillary, the involucre is possibly a genuine trimerous one, and we take the two small stipule-like leaves to be real stipules,—a point which the published character does not decide, though it is implied in describing the leaves as alternate.

Being one of the most important of recent contributions to North American Botany, this publication deserves even a fuller notice than we can here give it. A. G.

6. *Musci Fendleriani Venezuelenses.*—Among the collections made by Mr. Augustus Fendler in Venezuela, in 1854–5, was a very fine one of the Mosses of the region which he explored. It was purchased by the late Mr. Sullivant and in part studied by him, and drawings of a large number of the species were made by Mr. A. Schrader under his direction. With the exception of a single set, accompanying the drawings, which was bequeathed to the Herbarium of Harvard University, the whole collection was made over to Mr. Schrader, the draughtsman, to be distributed by sale among the bryologists. But, as a very large proportion of the species were new, it was desirable that they should all be authentically named, and the new ones described before the sets were offered. This has now been satisfactorily and most obligingly done by Dr. Karl Müller of Halle, the accomplished author of the Species Muscorum, and publication made in the Linnæa (xlii, parts 5 and 6), this giving the highest value to the collection. Mr. A. Schrader, of Columbus, Ohio (234 West State street), now offers these sets to botanists, at $12 for the 145 species, in good specimens, with a printed form of ticket bearing the numbers, and a copy of Dr. Müller's memoir (in Latin) in the Linnæa, enumerating and describing them. There are thirty-nine sets, all of equal completeness and value. To secure them early application should be made to Mr. Schrader. A. G.

7. *Dictionnaire de Botanique ;* par M. H. Baillon.—The first part of the second volume has appeared, containing articles from *Chlœnacées* to *Cistus*. No small part of the eighty pages are occupied by two articles, *Chlorophylle* and *Circulation*, each as it were a treatise. A. G.

8. *Miscellanea.*—The following are the more important botanical publications which have accumulated upon our table:

Transactions of the Linnean Society of London. Second Series, parts 5 and 6, of vol. i.—These contain Casimir DeCandolle's paper on the geographical distribution of the *Meliaceæ*, with a map; New British *Lichenes* by Leighton, with a fine plate; *Liliaceæ* and other petaloideous Monocotyledons of Welwitch's Angolan Herbarium, by Baker; New Zealand *Lichenes*, by Knight; the fine paper on the morphology of *Primulaceæ*, by Masters; a new genus of parasitic *Algæ* and conidial fructification in the *Mucorini*, by D. D. Cunningham; Fungi from Queensland, by Berkeley and Browne, and the Rev. George Henslow's memoir on self-fertilization in plants, which has been reviewed in this Journal at some length.

Nouvelles Archives du Museum.—The first volume of the second series begins with Decaisne's full and interesting monograph of *Ligustrum* and *Syringa*, with three plates. There are thirty-seven species of *Ligustrum*, and eleven of *Syringa*, including *S. Amurensis* and two allied species with rotate corollas, the genus *Ligustrina* of Rupprecht.

Rivista Botanica dell' Anno 1878, di Federico Delpino, 1879, Extr. Ann. Scientifico Italiano, Ann. xv.—A general review of botanical publications of 1878. Signor Delpino, now Professor of Botany in the University of Genoa, sends us also continuations of his valuable papers on dichogamy of flowers, and some other interesting essays, which we hope to review later. A. G.

III. Astronomy.

1. *Ephemeris of the Satellites of Mars for Oct. and Nov.*, 1879.—The following ephemeris, computed from Prof. Hall's elements of the satellites of Mars, gives the approximate position angles and distances of the satellites about the time of elongation. Only the times of those elongations are given which may be observed in America. On Nov. 1 the probable error of the computed angle of position is for Deimos $\pm 0°.6$ and for Phobos $\pm 8°.1$.

Deimos.

Date.	Wash. M. T.		Pos. Ang.	Dist.	Date.	Wash. M. T.		Pos. Ang.	Dist.
	h	m	°	″		h	m	°	″
Oct. 10	10	46	232·		Oct. 30	15	34	232·	
12	8	13	52·1	60 80	Nov. 1	12	0	52·7	66·88
13	14	31	52·		3	9	27	232·	
15	12	58	232·		4	15	45	232·	
17	9	25	52·	62·2	5	6	54	52·	
18	15	43	52·		6	13	12	52·	
20	13	10	231·		8	10	38	231·	66·6
22	10	37	51·3	63·73	9	16	56	231·	
23	16	55	51·		10	8	5	51·	
25	14	22	231·		11	14	23	51·	
27	11	49	51·		13	11	50	231·	
29	9	16	232·	65·	15	9	17	50·8	64·88

Phobos.

Date.	Wash. M. T.		Pos. Ang.	Dist.	Date.	Wash. M. T.		Pos. Ang.	Dist.
	h	m	°	″		h	m	°	″
Oct. 10	8	2	55·	24·5	Oct. 28	8	30	234·	
	11	53	235·			12	19	54·	
	15	41	55·			16	9	234·	
11	7	0	55·		29	7	28	234·	
	10	50	235·			11	17	54·	
	14	39	55·			15	7	234·	
12	9	47	235·		30	10	15	54·	
	13	37	55·			14	5	234·	
13	8	45	235·		31	9	13	54·	
	12	35	55·			13	2	234·	
	16	24	235·		Nov. 1	8	10	54·	
14	7	43	235·			12	0	234·	26·8
	11	32	55·			15	50	54·	
	15	22	235·		2	7	8	54·	
15	6	41	235·			10	58	234·	
	10	30	55·			14	47	54·	
	14	20	235·		3	6	6	54·	
16	9	28	55·			9	56	234·	
	13	18	235·			13	45	53·	
17	8	26	55·		4	8	53	233·	
	12	15	235·			12	43	53·	
18	7	23	55·			16	32	233·	
	11	13	235·		5	7	51	233·	
	15	3	55·			11	41	53·	
19	6	21	55·			15	30	233·	
	10	11	235·		6	6	49	233·	
	14	0	55·			10	38	53·	
20	9	9	235·			14	28	233·	
	12	58	55·		7	5	47	233·	26·8
21	8	6	235·			9	36	53·	
	12	16	55·			13	26	233·	
	15	46	235·		8	8	34	53·	
22	7	4	235·			12	24	233·	
	10	54	55·	26·0		16	13	53·	
	14	43	235·		9	7	32	53·	
23	9	51	54·			11	21	233·	
	13	41	234·			15	11	53·	
24	8	49	54·		10	10	19	233·	
	12	39	234·			14	9	53·	
	16	28	54·		11	9	17	232·	
25	7	47	54·			13	6	52·	
	11	37	234·			16	56	232·	
	15	26	54·		12	8	15	232·	
26	6	45	54·			12	4	52·	
	10	34	234·			15	54	232·	
	14	24	54·		13	7	12	232·	
27	9	32	234·			11	2	52·	
	13	22	54·		14	10	0	52·	26·2
						13	49	232·	

IV. Miscellaneous Scientific Intelligence.

1. *American Association.*—The twenty-eighth annual meeting of the American Association for the Advancement of Science was held at Saratoga, during the week from August 27th to September

3d. The President of the meeting was Professor George F. Barker of Philadelphia; the Vice Presidents, Professor S. P. Langley of Alleghany and Major J. W. Powell of Washington; the chairman of the Sub-section of Chemistry, Professor Ira Remsen of Baltimore, and of that of Microscopy, Professor E. W. Morley of Hudson, Ohio.

The officers of the local Committee were Dr. R. C. McEwen, Dr. J. L. Perry, Professor H. N. Wilson, Lt. Commander A. R. McNair, Professor L. S. Packard. They with the other gentlemen associated with them were highly successful in their arrangements for everything connected with the main purpose of the Association, as well as for the comfort and entertainment of the members. Under their auspices excursions were taken to Luzerne, to Lake George, to the Ausable Chasm, to Port Henry, and to other points of interest.

Of the various addresses delivered in the evenings, first to be mentioned is that of the retiring President, Professor O. C. Marsh, on the "History and Methods of Palæontological Discovery." This will be printed in the next number of this Journal. Other addresses were delivered by the Vice Presidents S. P. Langley and J. W. Powell on Friday evening, August 29; Saturday, August 30, by Dr. T. A. Edison on the "Electro-chemical Telephone."

The next meeting is to be held at Boston on the last Wednesday in August, 1880. The officers appointed for the meeting are: President, Lewis H. Morgan of Rochester; Vice President of Section A, Professor Asaph Hall of Washington; Vice President of Section B, Professor Alexander Agassiz of Cambridge; Chairman of Sub-section C, John M. Ordway of Boston; Chairman of Subsection E, Major J. W. Powell of Washington.

The following is a list of papers which were read or accepted for reading in the different sections.

I. *Mathematics, Astronomy and Physics.*

Experimental Determination of the velocity of light, A. A. MICHELSON.

The comet of 1771: investigation of its orbit, W. BEEBE.

Some observations on the depth of snow compared with the depth of the water it yields, J. BROCKLESBY.

Statement of generalization reached in question in intersection of circles and intersection of spheres: results stated, not the geometrical discussion, B. ALVORD.

Meteoric constitution of the sidereal universe: I. Cooling of the sun, B. PIERCE.—II. Cooling of the earth, id.

On a curious case of crystallization of Canada balsam, G. F. BARKER.—On the conversion of mechanical energy into heat by magneto-electric machines, id.

A general law indicating the location of planets, satellites, or annular rings around their primaries; also its utility, S. MARSDEN.

On experimental solution of a problem in the doctrine of chances, T. C. MENDENHALL.

Solubility of ozone, A. R. LEEDS.

Modification of the glass-plate polarimeter, A. W. WRIGHT.—Influence of light on the electrical conductivity of metals, id.

On the use of glass circles for meridian instruments and of glass bars for standards of length, W. A. ROGERS.—First results from a new diffraction ruling engine, id.—On a standard meter and its subdivisions into equal parts. id.—On the coefficient of expansion of nickel-plated bars, id.—On the tremors communi-

cated to transit piers and clock piers through the walls of an observatory building, id.

Observations of double stars, ASAPH HALL.

Star places, LEWIS BOSS.—Solar parallax from minor planets at opposition, id.

On determination of errors of form of the pivots of meridian instruments, C. A. YOUNG.

On the color correction of achromatic telescopes, WM. HARKNESS.

The past state of the world's metrology as bearing on the progress of science, F. A. P. BARNARD.

New methods of Photometry as applied to electric light, P. H. VAN DER WEYDE.

A table of remainders of 2^n to various prime moduli, E. P. AUSTIN.

On the identity of the lines of Oxygen with bright solar lines, as shown in photographs taken with increased dispersion, HENRY DRAPER.

On a trigonometrical view of the calculus, J. TROWBRIDGE.

On a resonant tuning fork, T. A. EDISON.—On the phenomena of heating metal in vacuo by means of an electric current, id.

A surface upon which the "non-euclidean geometry" finds its interpretation, A. W. PHILLIPS.

Test of faradic machines, F. R. UPTON.

Upon the methods of exploring the field of induction of flat spirals, A. G. BELL.—Upon residual induction, id.—Upon binaural audition, id.

Observations of the transit of Mercury, 1878, May 6, including a systematic search for a satellite, and measures of the diameter of the planet, D. P. TODD.

On the strength of American timber, R. H. THURSTON.

On explosive and detonating compounds, B. S. HEDRICK.

A study: forms of energy, J. D. WARNER.

II. *Chemistry.*

Action of ozone upon the coloring matter of plants, A. R. LEEDS.—Bleaching of sugar syrups by ozone, id.—Reduction of carbonic acid by phosphorus at ordinary temperatures, id.—Oxidation of carbonic acid by air over phosphorus at ordinary temperatures, id.

Household chemistry, ELLEN H. RICHARDS.

On the deterioration of library bindings, W. R. NICHOLS.—Observations on the variations in the temperature and chemical characters of Fresh Pond, Mass., id.—On an accidental contamination of a source of water supply, id.

Percentage of sugar in sap of the sugar maple, H. M. WILEY.—A modified method of collecting and measuring gases soluble in water, id.

Details of the construction of an apparatus for the analysis of gases, E. W. MORLEY.—Results of systematic analyses of air, designed to discover the cause of variations in the quantity of oxygen therein contained, id.

Preliminary notice of the revision of the atomic weights, F. W. CLARKE.

On graphite from the Ducktown copper mine, W. L. DUDLEY and F. W. CLARKE.

Method of coloring, cutting and polishing agates, etc., at Oberstein, Mrs. E. A. SMITH.

Note on a peculiar case of corrosion of the metal tin, J. W. OSBORNE.

On the limits of meteorological conditions governing the extension of beet root culture, W. MCMURTRIE.

On the action of caustic alkaline solution on glass, A. A. BRENEMAN.

A chemical examination of black ozokerite, C. G. WHEELER.

Exhibition of crystals of sapphire from the gem beds of Ceylon, with brief remarks, A. C. HAMLIN.

III. *Geology, Zoology, Botany.*

The succession of glacial deposits in New England, W. UPHAM.

On the thinning and absence of Upper Silurian and Devonian formation in Tennessee, J. M. SAFFORD.

Recently discovered cupreous veins at Blue Hill, Me., C. H. HITCHCOCK.

On the histology of insects, C. S. MINOT.—On the conditions to be fulfilled by a theory of life, id.—On the anatomy of Plathelminths, id.

A short biography of the Menhaden, G. BROWNE GOODE.

On the fertilization of Yucca, T. MEEHAN.

Philosophy of the pupation of butterflies, and particularly of the Nymphalidæ, C. V. RILEY.—The cotton worm in the United States, explanation of its work, and mooted points in its habits cleared up, id.

Homologies in the Lauraceæ, L. F. WARD.—Sexual differentiation in *Epigæa repens*, id.—Note on the movement of the stamens of Sabbatia angularis, id.

On a remarkable crinoidal form recently discovered in Tennessee, with exhibition of specimens, J. M. SAFFORD.

The fauna of the Lower Helderberg group, in relation of the Corals, Bryozoa and Echinodermata, JAMES HALL.—Notes upon the genera Fenestella, Hemitrypa, etc., id.—On the present condition of the work upon the palæontology of New York, id.

On some pre-Cambrian rocks in America and Europe, T. STERRY HUNT.—On the geology of Port Henry. N. Y., id.

The newly discovered cave in Luray, Page Co., Va., J. W. CHICKERING, Jr.

Gypsum sand, A. P. S. STUART.

The interoceanic canal problem, E. P. LULL.

On the progress of the second geological survey of Pennsylvania, J. P. LESLIE.

Notice of the occurrence of rocks representing the Marcellus shales of New York in Central Ohio, R. P. WHITFIELD.

Objects of sex and of odor in flowers, T. MEEHAN.

On the genus Pleurotomaria, W. H. DALL.—On the molluscan dredgings in the Gulf of Mexico and vicinity by the U. S. Coast Survey steamer Blake in 1877–9, id.

On the species of Bornean Orangs, with notes on their habits, illustrated by specimens, W. F. HORNADAY.

On the occurrence of microscopic crystals in the vertebra of the toad (*Bufo Americanus*). [With a note by A. A. Julien.] H. C. BOLTON.

On the anthracite coal fields of Pennsylvania and their rapid exhaustion, P. W. SHEAFER.

The development of the neuration in the wings of insects, as illustrated in the history of cockroaches, S. H. SCUDDER.

The bud-blight insect, W. S. BARNARD.

An apparatus for photographing natural history objects in a horizontal position, S. H. GAGE.—A method of demonstrating the thoracic duct in animals, id.—The inter-articular ligaments of the head of the ribs in the cat, id.

A new form of unpolarizable electrodes for physiological research, H. P. BOWDITCH.

A successful moth-trap, W. S. BARNARD.

Colors and defective vision, D. G. CROLY.

Some new or little known anatomical and physiological instruments and apparatus, B. G. WILDER.—Notes on the anatomy of the cat's brain, id.—On a cat's brain with the *corpus callosum* absent, id.

Some interesting insect habits and their development, W. S. BARNARD.

On certain forms of Muscæ Volitantes, J. W. OSBORNE.

On the Triassic rocks of New Jersey, G. H. COOK.

The Green Mountain anticlinal, C. H. HITCHCOCK.

On the geology of Bermuda, W. NORTH RICE.

The geological action of the Acid of Humus: I. On unconsolidated deposits. II. On solid rocks, A. A. JULIEN.

The genesis of the Serpentine of Reichenstein, etc., R. B. HARE.

On the surface limits of thickness of the continental glacier, in New Jersey, J. C. SMOCK.

IV. *Anthropology.*

The ethnical influences of physical geography, D. WILSON.

Archæological notes from Japan, E. S. MORSE.

Description of a polished stone implement found in Monkton, Vt., J. M. CURRIER.

The substances amber and jade, illustrated by remarkable specimens, Mrs. E. A. SMITH.

On the explanation of hereditary transmission, LOUIS ELSBERG.

The Ethnology of the islands of the Indian and Pacific oceans; illustrated by a large colored map, A. S. BICKMORE.

The sign language of the North American Indians, GARRICK MALLERY.

On some large and remarkable stone implements of the southern mound builders, F. W. PUTNAM.—On the pottery of the southern mound builders, id.

Superstitions among the ancient inhabitants of the Mississippi valley relative to the owl, to the rabbit, to serpents, to thunder, J. G. HENDERSON.—Lake Erie and the Eries, id.

Exhibition of archæological objects, S. S. HALDEMAN.

Archæology of Champlain valley, giving a general account of recent discoveries, G. H. PERKINS.

Consonantal expression of emotion, C. J. BLAKE.

2. *British Association.*—The meeting of the British Association—the forty-ninth—was held at Sheffield during the week from August 20th to August 28th. The exercises were opened on Wednesday evening, the 20th, by the Inaugural Address of the President, Professor G. J. Allman; devoted to "an account of the most generalized expression of living matter, and of the more recent investigations into its nature and phenomena." The attendance was not very large, but the papers were numerous and excited much interest; on the whole the meeting is described as having been one of decided success. The first of the evening discourses was delivered by Mr. W. Crookes upon Radiant Matter. It was illustrated by a large number of remarkably beautiful and brilliant experiments, performed on so large a scale that they were visible to an audience of nearly two thousand persons—this lecture forms the opening article in the present number of this Journal. The next meeting of the Association is to be held at Swansea, with Professor A. R. Ramsay as President. The meeting of the following year, 1881, is appointed for York.

3. *A Treatise on Hygiene and Public Health:* edited by ALBERT H. BUCK, M.D. 2 vols. 8vo. pp. 792, 657. New York, 1879. (William Wood & Co.)—This treatise consists of a series of twenty-five essays specially prepared by nearly as many contributors, most of whom are already known as specialists in various departments of sanitary science, medicine or surgery, or who are officers of boards of health. The essays treat of such special subjects as Infant Hygiene, Foods and Drinks, Food Adulterations, Water and Water Supply, Ventilation, Drainage and Sewerage in their sanitary aspects, the Hygiene of occupations, of camps and of marine service, Hospital Construction, Public Health, Vital Statistics, Public Nuisances, Disinfectants, Quarantine, etc., suitably edited and arranged.

As would be inferred the essays are of different degrees of excellence and completeness, and the treatise in a measure combines the characters of a hand-book of hygiene and a cyclopedia of sanitary science. As a whole, the work is well done. Some of the papers are particularly good; several of them are supplemented with a bibliography of the subject, and most of them are adapted to popular as well as professional readers. The treatise is a valuable contribution to sanitary literature in that it is a presentation of the subjects in the light of our present knowledge, by recognized authorities. W. H. B.